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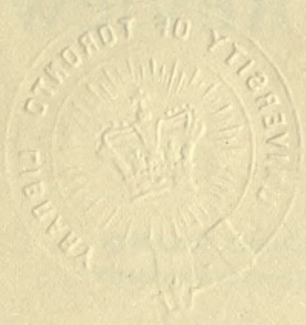

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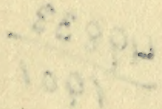

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THE PSYCHOLOGICAL REVIEW.

PSYCHOLOGICAL ATOMISM.

BY PROFESSOR HUGO MÜNSTERBERG,
Harvard University.

We know the overwhelming richness of technical means through which the naturalist seeks the elements of his material. He dissects the organism into organs and the organs into tissues, and inspects the stained tissues under microscopes until he finds the cells. We know the complicated paths which lead the chemist to the complete analysis of every substance into measurable quantities of chemical elements and those which lead the physicist to divide the ray of light or the sound or the electrical discharge into elementary movements. The corresponding scheme of the psychologist must be very different. As his material is not accessible to the dissecting knife or to the dissolving chemical solution, he has merely the internal microscope of attention at his disposal. He dissolves the mental state into its distinguishable parts by turning the attention to the one or the other element, increasing the vividness of faint elements which were perhaps at first unnoticed and isolating the single parts of the complex by this inner accentuation. Such an isolation allows the linking of the part with all those characteristic associations by which we call a mental state known to us and recognized. The element of the idea which in the unity of the mental state was merely a vague unrecognized shade of the whole complex appears under the searching light of the attention as a well-known acquaintance, perhaps as a muscle or joint or pressure sensation or as the feeling of a special organ. All

technical outer appliances are but guides and helps for this attention. Resonators may help us to give our attention to the overtones contained in the sound, but we do not acknowledge as present what cannot be made accessible to the searchlight of attention. No outer analysis of physical processes can be substituted for this internal inquiry; we may know that the ray of white light contains all the rainbow colors, but so long as no appliance has been invented to enable our attention to discover other light sensations in our sensation of white it would be absurd to say that the white sensation contains the red and the green sensations.

This self-observational analysis meets no difficulty of a theoretical character in so far as the ideas, those of perception and memory and imagination and reasoning, are in question. If we call the distinguished parts of ideas sensations, the sensational structure of the ideas has become a problem which for a perfect solution needs much effort still, but theoretical obstacles seem not to stand in the way. With regard to the non-ideational mental states, the emotions and volitions, the situation is somewhat different. Analytical psychology has not been idle in this field, to be sure; but what has been offered as a result of the inquiry seems less satisfactory and less convincing, above all less final than the work on ideas. The general trend of this modern analysis of will and emotion can be understood from one point: they are described as combinations of sensations, that is as combinations of such elements as we find in our ideas. Every part of these emotional and volitional experiences, as soon as attention is directed to it, shows itself as an element which is known to us as a part of ideas also and is thus a sensation according to the definition. This does not mean that will and feeling themselves become ideas but that the elements of both appear to be the same. I said this result seems unsatisfactory: the attitudes of our personality seem to us in the immediate experience of our inner life given in a way so absolutely different from the passive presentation of ideas that we are reluctant to believe that they represent merely two different kinds of combination of the same material. Long before we think of analysis we feel that the ideas are given to us as

objects, while our emotional and volitional life is subjective activity. And yet it cannot be mere chance that every new attempt of scientific inquiry shows ever new sensations in that section of mental life.

The solution of this apparent contradiction can be seen only from a higher point of view than that of empirical psychology. The solution must come from an acknowledgment of the fact that mental life can be brought under two different aspects. From the standpoint of our immediate inner experience our will and feeling are never an object but an activity felt to be sharply separated from our ideas, which come to us as objects. But in that state of felt activity we do not have possible material for psychology. Psychology imitates the work of natural sciences just in that it analyzes and describes objects. Will and emotion are in reality incommensurable with ideas and their elements; in that the instinct of the hesitating sceptic is quite correct. But in that real stage will and emotions cannot be material for psychology at all and to make them such analyzable and describable things they must somehow be substituted by objects: in place of the original will and feeling we substitute the attitudes and actions and excitements of the perceivable organism and in so doing we deal indeed with objects whose elements are sensations; in that, the instinct of the modern psychologist is correct. Whenever the really psychological point of view is taken the emotions and volitions are just as truly objects, and as such just as truly content of consciousness as are the ideas.

At a first glance, to be sure, some one may say: psychology is indeed obliged to conceive will and emotion as objects, but that obligation does not imply the necessity that these objects have the same elements which are characteristic of the ideas. But even this objection is untenable. Description involves communication, but we cannot communicate a psychical object directly, as it lies in the nature of the psychical state to be exclusively the property of one individual: I cannot show my inner state to any one else and no one can give me a direct share in the contents of his consciousness; we can suggest to each other an understanding of our intentions and inner attitudes but every one must produce them in his own private consciousness. The

outer physical world is public property which any two subjects share with each other and about which a direct communication, a common taking hold, is possible. We can therefore communicate a mental state only indirectly and only in so far as it is necessarily linked with a part of the outer world. But this is true solely in regard to the ideas and their parts, the sensations. Every idea is necessarily related to the physical object which is referred to by the idea, and every sensation refers in the same way to a special feature of the meant object. This relation exists for no other groups of objects, or rather, wherever such a relation exists we speak of ideas. To make psychological objects describable means, therefore, to conceive them as complexes of possible elements of ideas; and as the presupposition must be that the psychical world is finally describable and thus accessible to science, we can say *a priori* that wherever an inner state is felt which cannot be fully described to-day in terms of sensations there is to be found a problem for to-morrow; but it is settled beforehand that whatever the result may be it must be a result in the language of sensations. To say there is an indescribable feeling or impulse is never to announce a psychological result, but only an unsolved psychological problem. The total mental life is thus for psychology a complex combination of coördinated sensations, and that statement can be accepted even when science is still far from the point where all these sensations shall be identified and recognized. These sensational elements are different from each other in various respects. They show, first, a qualitative difference which may be as small as that between the pitch of two neighboring tones or nuances of color, or as large as that between tone and temperature sensation or color and smell sensation. They show, secondly, differences of intensity, as every quality may vary from the weakest impression to the most intense; and, thirdly, each sensation may be given in any degree of vividness. The original character of this last category cannot be denied and cannot be thrown together with intensity, as the weak sensation may be vivid and the strong one quite unvivid in our minds.

We stand now before the chief question of this inquiry: Are these sensations the ultimate elements of the contents of our

consciousness, or is that which we call a blue or hot sensation, a sweet taste, a tone C, a muscle sensation or a pain sensation itself a complex affair which consists of more elementary parts: in short, have we in the mind ultimate elements which are simpler than the sensations? It is the inquiry for a radical psychological atomism, and even those who would reject the proposed view that all psychological facts are complexes of sensations must acknowledge the right and importance of this question. Only the bearing of this question is for them not the same as for us, because for them it means only the inquiry about the ultimate elements of the ideas, while for us it means the question about the ultimate elements of the whole psychological life. It seems at first surprising that psychology in its modern form has hardly ever seriously raised this question, and has always stopped in its analysis as soon as the distinguishable sensations have been reached. Physics did quite otherwise: it never stopped at the point where the observation of the biologist or physicist or chemist found the last mechanically separable parts. Theoretical physics went far beyond that point, and saw its goal in a description of the physical universe according to which those cells and molecules and chemical substances are combinations of atoms which are unperceivable; and the atomistic theory of the universe which necessarily transcends empirical observation is to-day the basis of all natural science. Why has psychology never felt this demand which seems to physics so profound and so natural? I must here refer for a moment to philosophic arguments, as all these functions of the scientist, carried out by him instinctively and without philosophical motives, must have had their deeper reasons. We must ask whether the difference in the nature of his material suggested this modesty in the analyzing psychologist, while the physicist rushed without fear into a most radical atomism.

What is the ultimate meaning of all description? The empirical scientist may say that by the communication of the elements he gives an understanding of the object as it really is. But can we accept this from a higher point of view? The naturalist dissolves the objects into parts, but those heaps of separated parts are no longer the original object which he

started to describe. We may show that we can analyze a house into bricks and boards, a plant into cells, a chemical substance into several elements, but we have no right to say that those cells are a plant, those bricks a house, those elements the complex substance. What we really mean is only that we can transform the one object into those other objects. I can transform the house into single bricks and the plant into single cells, but I cannot transform the plant into bricks and the house into cells; that is what the description of the elements means. The enumeration of the elements does not approach the reality of the object nearer than the mere perception of the object as a whole does, but it allows us to judge of the possible transformations of the object; we learn what we have to expect from the object in the future, we understand the effects which the object must produce; that is, our descriptive enumeration of elements is intended to give us a causal understanding of the changes which go on. In other words, there is no description which is not a preparation for causal explanation, and what we call elements are only the foothold for our expectation of possible transformation. Description and explanation are inseparable. All progress of causal understanding means a progress in the full description of objects, and the descriptive search for elements has no scientific right to existence if it does not prepare a new understanding of causal connections. The claim of the naturalist that the world as physical object is made up of physical atoms is thus merely a special expression of the demands of physical explanation. Not a desire for an exact description for its own sake, but the desire for a consistent causal interpretation of the physical universe has brought about the atomism; the world must be conceived as a system of atoms to allow those expectations in regard to causal changes which modern physics demands.

The situation in psychology is in principle the same. We have said that there all analysis is based on the isolation which comes as a function of attention. But if our attention separates the different parts of an idea, we can say again that such a series of sensations is no longer the original idea; that group of separated sensations is a new content of consciousness, and if we

say that the idea contains those sensations as elements we really mean that by an action of attention we can transform the idea into that series of sensations. The passing over of the idea into a series of isolated sensations is the only fact which we seek to express by the statement that the sensations are parts of the idea. The idea of a painting can be transformed into a series of visual sensations, the idea of a melody into a series of tone sensations, while our attention can never expect to transform the melody into a series of visual sensations. Description means thus, in psychology exactly as in physics, the enumeration of those characteristics of the object which determine our expectations as to the transformations of the object. Ultimately then not the isolated object but its connections, its transformation into something else, is the material of the description. The analytic interest of psychology therefore can continue only so long as the discovery of elements means an understanding of the connection of successive mental states. Has psychology in that respect the same interest as physics in carrying out its analysis to the last possible elements?

The situation of psychology is here decidedly different from that of physics. All connection between physical objects is a direct one; one movement is the directly necessary cause of another movement. On the other hand no psychical process conceived as an object of consciousness is ever in itself the direct cause of another psychical process. No direct causality ever links two psychological contents. We may formulate generalizations of observed successions, but they never bring us that causal necessity which mechanics knows. Or we may think of the inner connections of our subjective attitudes, the inner connections to which our freedom and unity and ethical life belong, but then we are no longer dealing with psychological objects. As soon as we want causal connections between these mental objects we must seek an indirect connection instead of the direct one, that is, we must link the mental states with physical objects. Even the passing over of the idea into a set of sensations under the light of the attention is not a change which has a necessary character; we can never understand why the one must bring the other. It would lead us too

far here to ask why that is so, why physical objects can be connected directly, psychical ones indirectly only. The reasons lie in the nature of the object; the physical object, since it is an object for all, lasts and can never disappear, the psychical object, since it is an object only for the one subjective act, can never return; the new psychical object can only be like the old one, it can never be conceived as the same. All causality is based ultimately on identity, and in a world of objects which cannot have a material lasting from one act to the next no direct causality is possible; for the purpose of a causal connection it must be conceived as linked with the lasting material of the physical world. But as soon as we understand that all psychical explanation is dependent upon the connection of the psychical facts with the physical ones, it is clear to us that our conception of this psychophysical relation must determine the point to which the psychological analysis shall progress. An analysis which is not in the service of explanation is, as we saw, useless and meaningless. If we see now that explanation in psychology must always be indirect, based ultimately on the causal connection of physical processes, it is clear that psychology has no reason to investigate elements which have merely a psychological interest and no bearing on the psychophysiological explanation.

It was therefore not by chance but from an instinctive feeling of the general philosophical situation, that psychologists have so far given hardly any attention to the question whether we can push analysis beyond the limit of the sensations, simply because sensations alone have appeared as those results of psychical analysis which can be linked with simple physical processes. Sensation is not, as is usually said, the simplest element which psychological analysis can find, but it is that factor of the mental states whose corresponding physical stimulus in the outer world cannot be dissolved any further without ending the mental effect. We cannot say that the physical process which corresponds to the sensation is in itself elementary from a physical point of view; not at all. But any part of the stimulating process into which atomistic physics may decompose the stimulus would no longer correspond to a distinguishable part of the psychical state. The sensation is thus not the simplest mental

state but that simplest mental state which can be used for a psychophysical connection, as the further dissolution of its physical correspondent would no longer go parallel to the psychological analysis, and any pushing of analysis beyond the sensation would thus be useless for explanation and therefore theoretically unjustifiable.

This was the philosophical background of psychological analysis in the past. But have we a right to stop here? Is the physical correspondent of the sensation really the simplest process which can serve as a basis for psychological explanation? I do not think so. If our psychophysics were based on the connection between the sensation and its physical stimulus outside of the body, then certainly we could not go any further. If the sensation appeared in consciousness only when that stimulation outside of the body went on, then it would be absurd to divide the process still further, as we have acknowledged that a division of the physical stimulus means the end of the sensation. But we all know that modern psychology has worked out a further development of the system of psychophysical connection, according to which we substitute for the stimulus that physiological brain process which is causally produced by the stimulus. Thus as basis for the psychological explanation we link color, not with the ray of light, but with the process which such a ray of light produces in the occipital center of the brain. Has the situation not essentially changed by this physiological appendix to the psychophysical theory? The outer stimulus which corresponded to the sensation cannot be divided further without breaking up the sensation; but is that true for the physiological brain process which is produced by the stimulus and which is now the process corresponding to the sensation? We have been too long satisfied with the hypothesis that the elementary stimulus of the outer world excites merely the physiological unit in the brain which is represented by the isolated ganglion cell. Physiological psychology has had at no time the right to claim that the anatomical connections or the physiological experiments have demanded or even supported just this hypothesis. It was simply an expression of lack of knowledge. We cannot say that more recent inquiries have put clear facts in the place of hypothetical ideas,

and yet it may be claimed that everything points toward a much more complex idea of the physiological substratum of the sensation. We are becoming more and more accustomed to the view that the most elementary stimulus brings about a nervous excitement which grows from the periphery to the center like an avalanche and stimulates in the brain a whole area which may overlap the area stimulated by another isolated excitement. If thus the simplest idea is the accompaniment of a process in a multitude of cells, the question arises at once, Which psychical state corresponds to the process in a single cell? To say that each cell of the set brings about that same sensation which is observed as a result of the multiplication of the process would be utterly useless; the idea that the psychical process which corresponds to the excitement of the physiological unit is something simpler than a sensation is thus natural from the beginning. As soon, therefore, as it is recognized that the brain-effect of the indivisible stimulus is an easily divisible physiological process the sensation ceases to be the ultimate result of psychological analysis, since a still more elementary psychophysical unity can now be conceived as the last factor in the explanation.

Under these circumstances it becomes the duty of the psychologist to prepare the way and to ask how far the psychological facts in themselves suggest such a further analysis. I enter reluctantly into this new field, and yet I am convinced that the time has come when we need a psychological atomism just as much as an atomism of the physical universe. The ultimate psychical elements will be like the physical atoms in that they are merely constructions for the purpose of explanation, not perceivable objects; just as no naturalist has ever seen or touched an atom, no psychologist will ever observe a psychical atom in his consciousness. That must be the case, because every distinguishable part of the idea must correspond to a special characteristic of the object of the idea, and as long as this ideational correspondence holds true we call the element a sensation; a part of the sensation must be thus unperceivable and yet it may be clearly determinable and it is certainly in consciousness just as the unperceivable atom is in space; it remains unperceivable as it is indistinguishable in that complex unity

which we call sensation, while every sensation may be related to the psychical atom as the symphony to a single tone.

The psychological fact which stands immediately in the foreground of such considerations is the fact of the similarity of the sensations. Every sensation finds its place in the empirical system by its similarity with others; we bring all light sensations together in one group because each has with every other more similarity than with any sound or smell sensation; among the light sensations each again finds its place among those most similar to it, each color being placed next to the colors which stand nearest to it in the spectrum and also next to the least differing intensities of the same color tone. Thus there is known to us no sensation whose exact place in the manifoldness of mental life is not determined through its similarity with others. What is this similarity? At first glance it may seem sufficient to say that two objects are the more similar the more easily they can be mistaken for each other; and as this would be possible for absolutely simple objects the similarity of the sensations would not militate against their simplicity. But this possibility of confusion does not imply anything as to the content of the two objects; it refers merely to their position in the psychical connections. That two ideas can easily be confounded with each other means that both under special circumstances have an equal influence on the stream of mental facts. To report the influences of a mental object is no description of its structure; as soon as we begin to describe the content of similar objects we have a foothold only in the analysis of complex objects. There we call two ideas similar whenever their component parts are partially identical. Similarity from the point of view of description is community of parts; these common elements of our ideas may be outer sensations or sensations of bodily origin in the case of similar form or rhythm, and so on. The logical conclusion by analogy is that two sensations also are similar to each other only when they contain various component parts of which some are common to both. These parts are of course not sensations but inexperienceable factors like the atoms, and as we do not know a sensation which is not in some way similar to some other one, we can say that no known sensation is an ultimate element.

Just as the atoms of physics are conceived under the general conditions of all-space filling objects, so the psychical atoms must be conceived under the general conditions of consciousness. Therefore as no conscious objects can be twice together in consciousness, it may be said at once that these ultimate elements do exist or do not exist in a state of consciousness but that they cannot exist more than once. Thus even these elements cannot be used for the purpose of constructing the strong sensations from a large number of equal atoms of which perhaps a smaller number constitutes the weak sensations. No; ever new qualities must come together to form a new sensation, it may be a new nuance or a new intensity; in the stronger sensation must be a new quality which was absent in the weaker sensation. It seems inadvisable, however, to apply this treatment to the variations of vividness; a sensation has similarity with another of different quality or different intensity; but it has no meaning to say that it has similarity with the same sensation of a different degree of vividness. We must thus acknowledge that the change of vividness is a category for itself which cannot be eliminated by a reduction to varying combinations of atoms.

So far, then, the only description which we can give of our undivisible elements of psychical material is that they are absolutely dissimilar to each other; as long as two are conceived with the slightest similarity to each other we must acknowledge that the analysis was not really ultimate. Each of these absolutely dissimilar elements can pass through all degrees of vividness down to the point of disappearance. The interesting relations which exist between quality, intensity and vividness suggest, by the way, from the beginning, that the variations in vividness of the atoms may bring about not only changes in the vividness of the whole complex which we know as sensation, but also changes in quality or intensity. We could think, for instance, that the vividness of the sensation changes as soon as the vividness of all its component atoms changes equally, while the intensity or quality of the sensation changes as soon as the relation between the degrees in vividness of the various elements varies.

The study of the similarity of sensations thus allows us to

determine conceptionally the elements which we must postulate, but we do not know from it anything about their mutual relations. Only one thing is evident—that all the demarcation lines which existed for the sensations have now disappeared. The sensations were grouped just by their similarity; the psychical atoms, which have no similarity, are thus without the benefit of this parceling. But their very disappearance prepares the way for a deeper understanding of the relations between the sensations, as the similarity of sensations of different senses then offers no difficulty; the similarity between smell and taste, or between touch and muscle sensation, and so on, appears then not different from the similarity of two tones.

Where shall we find a foothold for the construction of the relations which exist between our postulated elements? Again we shall of course be guided by the analogy with observable processes of the complex states. An idea is related to other ideas by associations and inhibition; it awakes other ideas or suppresses other ideas. We have no reason to believe that this power of mental contents is dependent upon their complexity; we can thus demand the corresponding possibility for our elements; they may awaken other elements or may inhibit them. What can we know about the special facts of this kind in our atomistic miniature psychology? With regard to association, of course, we can claim that every real sensation shows us a case of such association of atoms; thus the analysis which we attempt on the basis of similarity merely expresses in a disintegrative form that which our atomistic association laws would express in a synthetic form. We have the results of associations in every experience of sensations before us; we do not need anything further in that respect. Quite differently with the indispensable correlate, inhibition. How can we ever find out anything about the inhibitory powers of our elements? The one strong light which seems to fall on this difficult problem appears to me to come from the fact of fusion of sensation, a fact which has itself only in recent years attracted the attention of specialists. We consider two mental contents as fusing when their simultaneous appearance renders their separation difficult. Even the unmusical person hears two tones when two

tones in the interval of a seventh are given; but it happens that he sometimes takes them for one tone when they are in the interval of the fourth, and more frequently with the fifth, and almost always with the octave; that is, the octave fuses most, the fifth less, the fourth less, the seventh still less than that, and so on.

The fusion of tone sensations is the easiest to study, but other sensations show the same relations. Every one knows how strongly smell and taste sensations fuse, or touch, muscle and joint sensations, or color sensations with colorless light sensations, or touch and temperature sensations, and so on. Of course if we say that fusion is characterized by the difficulty of recognizing the two fusing ideas or sensations we do not give a real analysis of the processes but speak merely of its mental effects on our associated judgments. But we can perhaps try to analyze the process itself. In every fusion each of the fusing parts becomes less recognizable because each part is deprived of something of its own character; with respect to its atomistic aspect we say that each of the component parts loses some of its elements. The sensation *A* may contain the atoms *abcd*, while sensation *M* contains the atoms *mnop*. We say that *A* and *M* do not fuse if *abcd* and *mnop* can coexist, while we have a small degree of fusion as soon as *A* suppresses, for instance, the *p* in *B* and the *M* suppresses the *d* in *A;* the result in the following together of *A* and *M* is then *abcmno;* and if merely *abmn* remain the fusion is more perfect. In this way fusion transforms itself into the inhibition of psychical atoms and allows a deep insight into the working mechanism of the ultimate elements. But from this point of view the inhibition of the elements means nothing but the decrease and association the increase of their vividness, so that this group of determinations also is reducible to the conception of vividness.

We thus have for the atomistic constitution of our psychical world the following suggestions: First, we know that they are elements which, while under the general conditions of consciousness, are different from our sensations; they are absolutely dissimilar to each other; they can vary through all degrees of vividness; but they are all coördinated, not belonging to any

special groups according to our special senses, and they are the material for building up in their qualitative manifoldness both the so-called qualities and the intensities of the sensations. Their existence must be worked out from the similarity of the given sensations. Secondly, these atoms can pass not only through all degrees of vividness themselves, but they can influence the vividness of other atoms. They may increase the vividness of other elements, that is, produce associations; the nature of these associated groups of atoms we can work out from the given sensations which represent all the known groups of such associations. On the other hand, they may decrease the vividness of other elements, and the facts concerning this suppressing influence we can work out from the empirical facts of that partial inhibition of sensations which we call fusion. The similarity of sensations thus shows us the way to the elements themselves, while the variety and the fusion of sensations show us the way to the mutual influence of the elements. All these influences are reinforcements and suppressions of vividness, and thus we can reach from the empirical study of the sensations, especially with regard to their similarity and fusion, a system of psychical atoms which are absolutely dissimilar to each other, but which stand in a mutual relation of increase and decrease of vividness. The different degrees of this mutual influence are then, of course, a natural basis for a systematic order of the manifoldness which cannot be ordered according to similarity.

This new world of atoms in psychology—that is, of really indivisible elements of mental facts—thus answers the same purpose which the physical atoms answer, but on account of the unlikeness of the material their character is different in every respect. The physical atoms are all qualitatively alike, that is similar in the highest possible degree, while the psychical atoms are absolutely dissimilar. The equal physical atoms get their individuality from their position in space; two atoms are two, not because they are different, but because they are in different places; in psychology two atoms are two, because they have qualities that cannot be compared. In the physical world all the processes of the universe are to be expressed in one function of the atoms, namely, movement, a change of place with change of

time; in psychology all processes of the psychical world are to be expressed in the one function, change of vividness. All the energies by which the physicist explains the mutual influences of bodies are functions of movement, and all the associations and inhibitions and fusions by which the psychologist explains the mental life are functions of vividness. The system of qualitatively similar elements in space which affect each other merely by influence on their movements, and on the other side the system of absolutely dissimilar elements in consciousness which affect each other by influence merely on their vividness, are thus two logically equivalent constructions which have equally the character of an ultimate systematization of the two worlds of objects.

Of course, we must not forget that on the psychical side the word 'influence' has not the same meaning as on the physical side; it has no real explaining power, as we have seen, till the psychical relation is linked with a physical one. But at this point everything seems ready for an adjustment. As soon as we conceive such an ultimate psychical element as the accompaniment of a single ganglion cell we understand without difficulty its associative and inhibitory influence on other ultimate elements. We must simply get rid of that unbased idea that a sensation has one physiological unit as its basis. In the beginning of physiological psychology it was quite usual to think that the whole idea of an object corresponded to one cell. At present this is corrected: we know that each idea contains many sensations and we give a cell over to one sensation. In the same way we must understand that every sensation is a very complex system and that only its elements, the psychical atoms, can rightly be correlated with the physiological units. All the difficulties in the way of the physiological foundation for the similarity and fusion of sensations, and so on, will then disappear and we shall have a really consistent system of psychophysics.

Only the one point must always be kept in mind that even these ultimate psychological elements can never be ultimate metaphysical realities. We have seen that psychical atoms, just like physical atoms, are constructed for the purpose of understanding the connections in the two systems of objects

which we perceive, but the one principle which we recognized from the beginning was that the aspect from which mental life appears at all as a psychological object is not the aspect of reality and life. In our life the inner experience is a felt system of will relations and a metaphysically ultimate factor must thus always lie in this immediate reality which we have left behind us when we conceive the world as physical and psychical objects. The psychical atoms are thus not more true as philosophical realities than any other parts—as, for instance, the sensations, or the molecules—both are merely constructions for logical purposes; but with these purposes constantly in mind, we must begin to see that the sensations are unsatisfactory as ultimate conceptions, and that they must be replaced by psychical atoms if psychology really desires to become worthy of its great older sisters, the natural sciences.

SHADOW IMAGES ON THE RETINA.

BY F. H. VERHOEFF, PH.B., M.D.

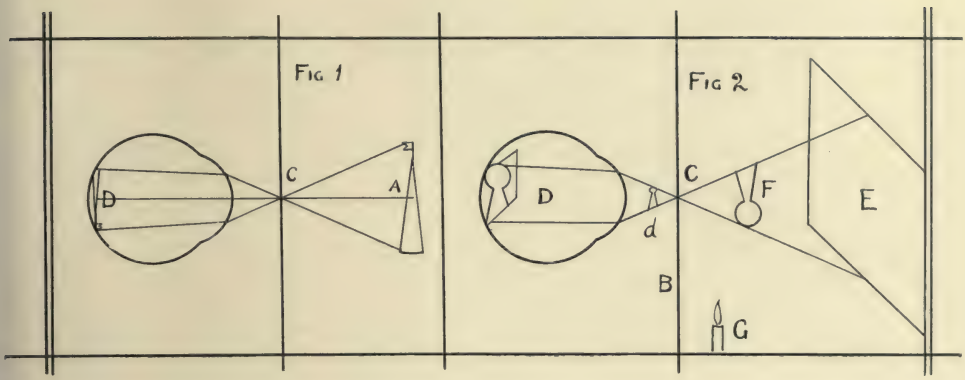
[From the Physiological Laboratory of Johns Hopkins University.]

In connection with the shadows thrown upon the retina by a small object, a pin for example, placed within the focal distance, I have recently noticed a striking optical phenomenon that is the exact opposite to the well-known pin-hole shadow experiment. The usual pin-hole experiment is produced by pricking a small hole in a card which is then held before the eye, but within the point of distinct vision. Under these conditions, if a pin is held between the hole, which serves as a source of light, and the eye, it casts an erect shadow upon the retina, and this shadow is projected as an inverted image of the pin.

This experiment was described and explained by Le Cat[1] in 1740. It is possible, however, that he was not the first to describe it, for he does not, in his work, claim that the experiment was original with him. The following is a translation of his description (of Plate I., Figs. 1 and 2) of the phenomenon:[2] "Without betaking yourself to a dark room, put before your eye *D* (Fig. 2), a black card *B*, pierced by a pin-hole, *C;* place opposite to and beyond this hole a very light body, such as a sheet of white paper, *E*, illuminated by a candle, *G;* place now a pin, *d*, before your eye, *D;* you will see with surprise the pin inverted, and on the other side of the hole, *F*. Let us see how this reversal and this transposition comes about. You understand that the images of external objects in passing through a hole (Fig. 1) are inverted and are depicted thus inverted * * * in the eye, *D;* it is the same with the images which pass through the single pin-hole,

[1] Le Cat, Traité des sens, Rouen, 1740, p. 297 (Helmholtz).

[2] Le Cat, Traité des sens, Paris, 1767, p. 507. I have not been able to obtain the earlier edition referred to by Helmholtz.



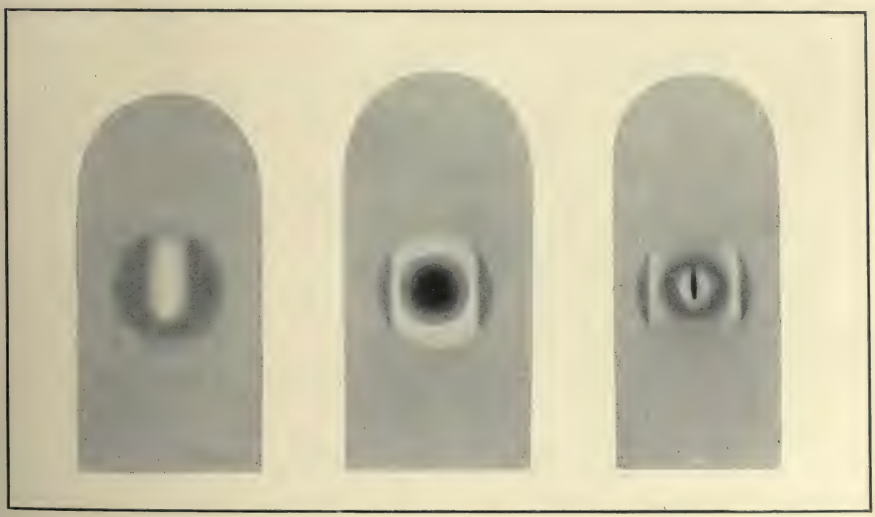


FIG. 6. FIG. 8. FIG. 9.

Verhoeff on Shadow Images on the Retina.

C (Fig. 2), and which are depicted in the eye, *D*. At the spot where the upright pin, *d*, is placed, the images are already inverted; now this pin meeting these inverted images arrests the rays which correspond to it and produces consequently in these images a lack of rays, a shadow of the figure of a pin; the pin in the midst of the inverted image is upright; the image of the paper, *E*, will then be depicted at the fundus of the eye as inverted, having in its middle a shadow of the pin in a correct position; now one perceives as upright objects which are inverted in the eye and reverses those which are upright, there will be seen then the external object *E* in an upright position and the shadow of the pin inverted; moreover, this pin, or rather this shadow of a pin, will be seen beyond the hole at *F*, because the pin which is seen is really but the shadow produced in the image of the external object *E;* this phantom pin ought then to be referred to the external object *E*, and be seen beyond the hole."[1]

Le Cat is entirely correct in his explanation so far as he goes, but he fails to mention the importance of the position of the pin-hole with respect to the conjugate focal point of the eye, and he also fails to explain fully why it is necessary for the pin-hole to be of small size. The mention of the white sheet of paper, *E*, in the explanation is unnecessary and also misleading, since it distracts attention from the essential points involved.

J. L. Tupper, in a letter to Professor Tyndall, published in

[1] Scheiner (Oculus, 1619, p. 49) described an experiment similar, in principle at least, to that of Le Cat. The following description of it is taken from 'Priestley on Vision,' Vol. I., p. 114, 1772: "If an object be suspended in a small hole made in a thin board, and an eye, situated in the dark, look through the hole at a number of torches or other luminous bodies, he observes that as many small objects will be seen as there are torches, etc. In fact, it is the shadow of the object made by each of the luminous bodies that is cast upon the eye."

In the diagram given by Scheiner the hole was represented as a comparatively large one—much larger than the pupil—and hence it could have nothing to do with the experiment. It is essential that the torches should be out of focus, a fact evidently unknown to Scheiner. The torches act as small sources of light, corresponding to the pin-holes in La Cat's experiment, but since they are beyond the conjugate focal points of the eyes, the shadows of the objects that they throw upon the retina are inverted, and hence appear erect. This experiment should not be confused with the experiment of Scheiner referred to further on in this article.

the *Philosophical Magazine*[1] for 1870, gave what he thought was the first correct explanation of the phenomenon. He was, however, evidently unacquainted with Le Cat's work on the subject. Tupper's letter was called forth by an entirely erroneous explanation given by Michael in No. 251 of the 'English Mechanic.' Tupper states the essential explanation in a few words when he says of the experiment, 'it negatives delineation by cones and admits of it by single rays only.' This is not, of course, strictly true, since it would be impossible to obtain a hole through which only single rays could pass and still give enough illumination to make the shadow evident, but the more nearly this condition of affairs is approached the sharper is the shadow. For this reason, therefore, it is necessary that the pin-hole should be small.

Le Conte,[2] in 1871, published a short article in which he takes exception to Tupper's calling the impression of the pin on the retina an image, Le Conte insisting that it is a shadow. He says: "Now, there is no doubt that this explanation is substantially correct; but it would have been much clearer if Mr. Tupper had distinctly expressed the fact that the retinal impression in his experiment is not an image, as in ordinary vision, but a shadow. Mr. Tupper seems to have perceived the distinction but has not kept it in his mind, and hence some confusion in his deductions."

Tupper thought the experiment disproved the law of visible direction and gives the following in support of his view: "A second deduction regards the law of visible direction by which it is said an object is seen in the direction of a perpendicular to the retina at the object's point of incidence; and it is cited by its promulgator to prove the sense of vision primary and unacquired; therefore, its operation should be absolutely efficient and unindebted to experience, unconditionally indicating the direction of the object. But the law proves fallacious, the object not being in the direction of the perpendicular; it is seen where it is not. That it is seen under novel and inexperienced conditions cannot be urged without admitting experience as a

[1] Vol. 39, Fourth Series, p. 423

[2] Joseph Le Conte, *Philosophical Magazine*, London, Vol. 41, p. 266.

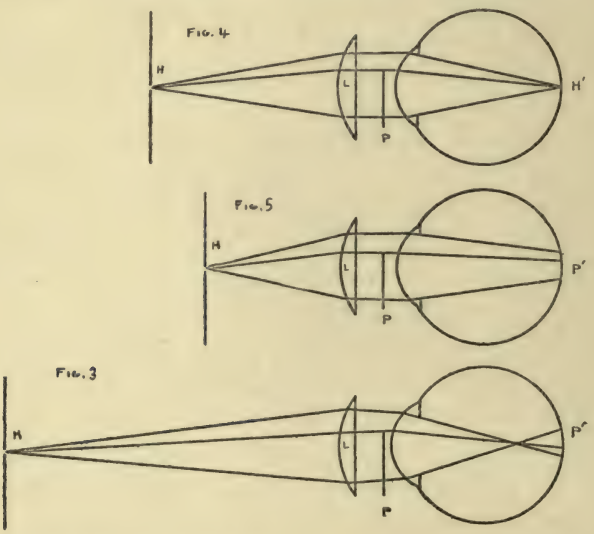
prerequisite to sense of direction." On this point Le Conte says: "The experiment confirms the law, as indeed does every phenomenon of vision. The law of visible direction is that every point of the retinal impression is referred to the field of vision along a line passing through that point and through the optic center. * * * This gives the pin inverted as we actually find it."

Le Conte was of course undoubtedly right in his statement. Tupper evidently did not have a very clear idea of what the law of visible direction is. It simply relates to the direction along which an object is apparently seen, not to the actual direction of an object, and is consistent with either theory of vision, the acquired or the congenital. The phenomenon not only confirms, as Le Conte says, but it proves the law of visible direction for it shows that no matter what may be the position of any retinal image or shadow, its apparent position is indicated by lines drawn from the image and through the optic center of the eye. As a rule of course visible direction corresponds to actual direction, but this is not necessarily true where the objects are smaller than the pupil and are out of focus.

Tupper makes another mistake when he says that a pin seen *through* a pin-hole is uninverted on the retina. Le Cat showed that this was not the case in his diagram (Fig. 1). Le Conte also rightly says that vision through a pin-hole is in all respects similar to ordinary vision. In this connection, however, Le Conte makes a statement which is really misleading, for he says, comparing the retinal impression in pin-hole vision to that in the experiment under discussion, "the one is a *true image*, the other a *shadow;* the one is *inverted*, the other *erect*." If Le Conte meant to imply by this that the reason the latter should be called a shadow is because it is erect, he was in error, for I shall show that it depends altogether upon the position of the pin-hole with regard to the conjugate focal point of the eye as to whether the impression is inverted or even as to whether the impression occurs at all. However, the term shadow is very appropriate, although I think shadow image would be still better. Le Cat, Tupper and Le Conte seem to have entirely overlooked in their respective explanations the importance of the position of the

pin-hole. Le Conte in his work 'Sight'[1] claims that he first explained the phenomenon and refers as proof to the article just mentioned. As a matter of fact he added little to what Le Cat and Tupper had already said.

To show the dependence of the position of the projected image upon the relative position of the pin-hole, the experiment may be modified in the following interesting way. If for instance the eye is focused for the near point and the pin-hole is held beyond this point, the shadow of an interposed pin will appear erect. Under these conditions if the pin-hole is grad-



ually brought closer to the eye, the image of the pin becomes more and more blurred, and finally disappears when the hole is at the near point, that is, when the eye is accommodated for the source of light. If the hole is brought still nearer, the original experiment is reproduced, the image of the pin again appears: but is now inverted.

[1] Joseph Le Conte, Sight, p. 87, New York, 1881. In the edition of 1897, p. 74, Le Conte says: "This phenomenon was explained by the author in 1871. See *Philosophical Magazine*, Vol. LXI., p. 266. It had, however, been previously explained by Priestley, but forgotten (*Nature*, Vol. XXIV., p. 80, 1881)."

I find that Priestley (Priestley on Vision, Vol. II., p, 725, London, 1772), briefly summarizes Le Cat's description of the phenomenon, referring to 'Traité des Sens,' edition of 1744, and gives all credit to Le Cat.

This experiment is rendered easier if the accommodation is assisted by a convex lens, one of about ten diopters being very convenient. The lens is held close to the eye and the pin in front of the lens, or better immediately behind it. Then if the pin-hole be placed beyond 10 cm. from the supposedly emmetropic eye, it will appear out of focus and the shadow will be upright. As it is brought closer, the phenomenon just described takes place. The accompanying figures (Figs. 3, 4, 5) explain this experiment. In each figure, *H* is the pin-hole, *L* the lens, *P* the pin. In Fig. 3, *H* is beyond the focal distance of the lens and the shadow appears erect. In Fig. 4, *H* is in focus at H′ and no shadow is formed. In Fig. 5, *H* is within the focal point and the shadow appears inverted. In place of a pin-hole, a small white spot on a black surface may be used as the source of light.

I shall now describe the phenomenon referred to at the beginning of this article. If a sheet of white paper or a piece of ground glass is taken, and a small black spot is made upon it about the size of a pin-head, and this spot is used in place of the pin-hole in the above experiment, a white streak will be seen crossing the black spot (Plate I., Fig. 6). The phenomenon is best obtained without a lens, and is not an easy thing for most persons to see, since it requires that the accommodation shall be relaxed at will. I shall for convenience speak of this light streak seen crossing the black spot, as a white shadow. This white shadow behaves in exactly the same manner as does the black shadow in the pin-hole experiment—under the same conditions it may be seen upright, inverted, or be made to disappear. The experiment should be conducted in a good light, and a piece of white paper not less than five centimeters square should be used. This phenomenon may also be obtained with a photographic camera.

The explanation of the phenomenon is not quite so simple as in the case of the black shadow. The explanation suggested is illustrated in Fig. 7. This figure represents an eye accommodated for parallel rays; *AB* is the diameter of the black spot and *PP′* is the cross section of the pin. The exact relative sizes of spot and pin are not represented, because the lines would

then have to be drawn so closely together that they would not be seen clearly. Following the usual optical constructions, it will be seen that the black spot causes an area of diminished brightness, represented in section as $B'A'$. The lines drawn from AB denote the rays which would have come from it had it the same luminosity as the rest of the visual field; the lines BC and BD represent the limits of the rays stopped by B, and the lines AC and AD represent the limits of the rays stopped by A. BC would strike the retina at B' and AD would strike it at A'. The continuations of AC and BD are not drawn, but would fall between B' and A', as would all the rays from points between A and B. Hence $B'A'$ would evidently have less illumination than the rest of the field, but would still be illuminated to a certain extent, since all rays parallel to the primary axis of the eye would fall upon its center, and the rest of it would be illuminated by rays parallel to the secondary axes which strike it, of course, at every point of its surface. Now if the pin PP' is put in front of the eye, there are still more rays cut out of the general illumination, PCI and $P'DN$ denoting their limits. The pin will therefore cause a diminished brightness, extending from I to N, and this is seen to include $B'A'$. But the rays included between APT and $BP'F$ have already been cut out of the field by AB, and therefore FT will have

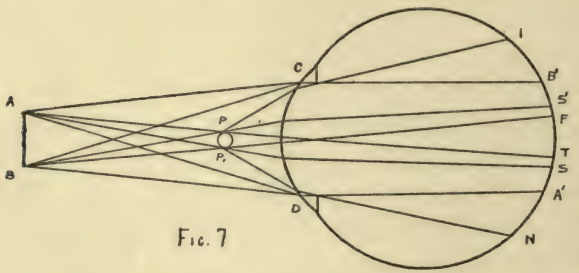


FIG. 7

the same illumination as before, and since $B'S$ and $A'S$ have less illumination than before the pin was placed in position, FT will, by comparison, appear as a light band cutting the darkened area $B'A'$. In the figure, $B'A'$ and FT represent, of course, only cross sections of areas. A figure representing

the pin in longitudinal section may be drawn by a construction similar to Fig. 7.

The white shadow is not absolutely clear cut because the edges shade off, owing to the fact that the original illumination of FS' and TS is gradually diminished from F and T outward until the maximum diminution in brightness is obtained extending from S' to B' and from S to A'. Since the length of the pin is greater than the diameter of the spot, a certain number of rays which would have fallen on FT are cut off and there is an actual *slight* diminution in the illumination of FT.

That physiological contrast plays a part in the perception of the white shadow is indicated by the fact that the area it occupies seems brighter than it did before the pin was brought across it, and is further shown by the fact that a red spot will give a faintly blue or green shadow. In this case, $B'A'$ (Fig. 7) would have an area of mixed red and white light, with the exception of FT, which is really white since the red rays are prevented from reaching it by the pin.

It is obvious that one of the most important factors in the formation of the shadow is the amount of illumination of $B'A'$, for it is the decrease in the illumination of the outer parts of $B'A'$ that makes the white shadow perceptible, and if the illumination is already very slight, even a total lack of light in certain parts would not be noticed. It is hardly necessary to state that the illumination must not come from the spot—the blacker the spot the better. If the black spot were in focus its image on the retina would be entirely black and the pin could not, for this reason alone, have any influence upon it, except perhaps to make the whole spot look lighter since it would diminish the brightness of the rest of the field.

If, in the place of a small spot, a sufficiently large one is used, it will be noticed that on relaxing the accommodation the center (of its image) appears much darker than the rest of the image. This is due to the fact that as the spot is made larger it cuts off more and more rays which would otherwise have reached the center of its image, and when it is as large as the pupil it cuts off all the rays parallel to the primary axis, thus producing a small spot in the center of its image totally devoid

of light. But even when the spot is much less than half the size of the pupil the image appears darker in the center, since many rays will even then be cut off from the center of the image. This circumstance explains the fact that if a moderately large black spot is used for the shadow experiment, the white shadow of the pin is broken by a dark central spot into two white lines, somewhat resembling crescents, and giving the appearance represented in Fig. 8. If a large black spot with a small white one in the center is used, the appearance of these two crescents may be obtained, and in addition the usual black shadow is seen over the central white spot (Fig. 9).

As to the diameter of the pin which can be used the theoretical limits are from a geometrical point to a diameter equal to the distance between the lines AC and BD, measured at the position the pin is to occupy. These do not indicate practical limits, however, for a very small pin does not cut off enough light to make sufficient difference in illumination on the retina, and in case of a large pin too much outside illumination is cut off to make the phenomenon apparent. As the diameter of the pin is increased it cuts off more and more rays from the center of $A'B'$, and by the use of a moderately large pin and black spot together with a convex lens I have been able to obtain a very faint, ill-defined dark line in the middle of the white shadow. It is possible, however, that this may in part be due to contrast. The size of the shadow for a fixed size and position of pin varies with the size of the spot. The distance the spot is held from the eye changes inversely as the actual diameter of both the image of the spot and the shadow, but the relative proportion of shadow to image remains constant. Other things being constant the size of the shadow varies with the distance of the pin from the eye, but not in a simple relation. The size of the shadow varies also with the size of the pin, but no matter how small the diameter of the pin its shadow can be no smaller than the diameter of the image of the spot when it is in focus. These statements apply when the eye is accommodated for parallel rays, and they seem to be borne out by practical tests.

The explanation of the erect image obtained when the spot is held beyond the conjugate focal point of the eye, and of its

disappearance when the spot is held at this point, simply involves the crossing of the rays as in the case of the pin-hole experiment. These changes are most easily obtained with a convex lens, but it should have a diameter so large that it does not shut out any rays from the eye.

When the eye is accommodated for near points, it is no longer true that FT (Fig. 7) has practically the same illumination as before the interposition of the pin, but the general principle that the illumination of FT is less altered than that of IN holds good. When the eye is so strongly accommodated that the pin itself is brought into focus, FT becomes totally devoid of light and coincides with IN, which is now the sharply defined image of the pin, while the rest of the blurred image of the spot AB is unaffected in any way. When the eye is accommodated for a point between the pin and the eye the white shadow again appears, since IN now becomes larger than FT and the illumination of the latter is diminished less than that of its immediate surroundings.

If in place of a spot a black line is used, the pin will produce a white line running down the middle of its image; the shadow in this case is more marked and more easily obtained than when a small black spot is used. Two lines drawn at a slight angle to each other will give a bend in the white shadow at their intersection, but the shadow leaves one or both of the lines at a certain distance from this point. When a narrow red line is used instead of a black one, and a lens is employed to aid the eye, the white shadow takes on a greenish or bluish color. By the use of a lens a narrow line may be so blurred as not to be seen, but a pin in front of the eye decreases so much the amount of light reaching the lateral portions of the image on the retina as to produce the appearance of a well-defined white shadow. The same thing is also true for a point.

Scheiner's experiment, in which two pin-holes placed close together are held before the eye and a double image of a pin is produced, may be explained in the same way as the white shadow just considered, since the two images obtained of the pin may be regarded as one image with a white shadow down its middle produced by the portion of the card-board between he two pin-holes.

When simply blurring a line by relaxing the accommodation, I have noticed that if the line is not too large I can always obtain a white line running down its middle and within this line a faint dark line. In the case of a small black spot, I obtain a white spot in the center of its blurred image. I think this must be due in most part to the denser and probably less transparent nucleus of the crystalline lens shutting off more rays than the rest of the refractive apparatus of the eye. This appearance is not produced by a photographic camera and hence must be mainly due to some such peculiarity of the eye as that suggested. It is possible, however, that the positive aberration of the eye may play some part in its production.

DIFFUSION OF THE MOTOR IMPULSE.

BY CLARK WISSLER AND WM. W. RICHARDSON.

The acquisition of voluntary control has always excited interest because of the peculiar problem presented and because of the feeling that its solution would put us far on the road to an understanding of the real nature of volition. It is a matter of common belief that we learn new movements by eliminating the unessentials from a veritable broadside of motor discharges, but science offers no satisfactory explanation as to how these unessentials are finally eliminated. It is considered logically impossible for voluntary to precede involuntary movement, since there must be a copy, or an idea, of the movement to be made before we can will to make it. Yet Baldwin insists that such a copy may be primarily visual, auditory, etc., instead of kinæsthetic, thus making imitation an important factor in the genesis of voluntary movement; but this copy is very imperfect and the corresponding kinæsthetic idea extremely vague, naturally resulting in a broadside of motor discharges, containing possibly the movements or some elements of the movements desired. This is the theory of excess.

It is quite certain that motor nerve elements evolve from the central system, and thus the order of development in a limb is toward the digits, or from fundamental to accessory. A great many writers have called attention to the phenomena of excessive or diffused movements when any part of the muscular system is working under great stress or extreme fatigue, or when executing unfamiliar movements; and it has been assumed that this diffusion is closely related to the so-called 'excess discharge' from which the more specialized movements are gradually evolved.

This paper is a report of some experiments with these diffused movements in normal unfatigued activity of the arm muscles.

The muscular anatomy of the arm is very complex, and though the positions of all the muscles are well known the action of many of them is yet a matter of dispute. Mechanically considered, the arm is a collection of levers capable of working in many combinations. This is the secret of its wonderful adaptability. One of the most complete series of levers is that presented when the arm, hanging freely at the side with the palm toward the thigh, moves in a plane parallel to the median plane of the body. In this position movements may occur at the shoulder, elbow (flexion), wrist (abduction) and at the knuckles (abduction of the first and second fingers). We have here a compound lever in which resistance could be applied at the end of the finger by movements of any one, any two, any three, or all of the parts in unison. All the levers in this series are represented by muscles at different levels, lying just beneath the skin, thus affording special advantages for observation. Beginning at the shoulder, the muscles chiefly engaged in the movements are the deltoid, biceps and brachialis anticus, extensor carpi radialis longior and extensor carpi radialis brevior, and abductor indicis.

The method of experimentation is quite simple and makes use of familiar apparatus. Tambours were placed over the muscles, so as to record their myograms upon a kymograph. To get myograms of the abductor indicis it was necessary to put the hand in a light clamping apparatus to serve as a firm support for the tambour. Some preliminary experiments revealed the fact that when the abductor indicis was contracted so as to move the unobstructed finger quickly through an arc of about 30°, secondary contractions of the arm muscles in the series occurred, but not at the same instant.

The limitations of apparatus made it necessary to study the muscles in pairs. Similar tambours were placed upon the biceps and forearm as previously stated. The time line was furnished by a 100-v. fork. The left arms of *R* and *W* were used in these experiments and records were taken at irregular intervals during a period of two months. The object was to determine the order of the secondary contractions of the two muscles of the arm as a result of the primary contractions of the abductor indicis. The abductor was not weighted, but simply

abducted the finger. The subjects made the contractions in response to a signal from the operator. Fig. 1 presents a typical

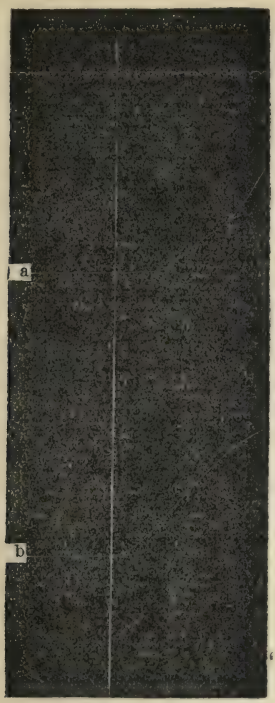


FIG. 1.

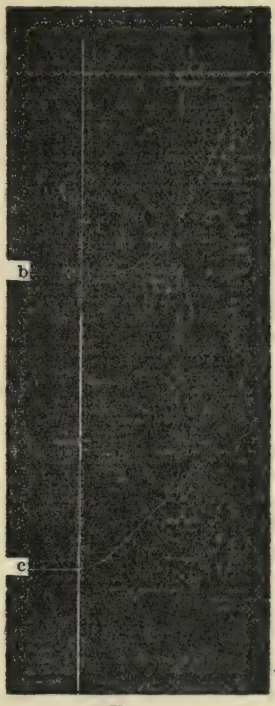


FIG. 2.

record. Here it is plain that the extensor muscles of the forearm contract sooner than the biceps. The average time difference for all the records was .016 sec. M.V. .004. In the cut, *b* is the tracing from the extensor carpi radialis longior, and *a* that of the biceps.

The tambours used in the above experiments were shifted to the forearm muscle and the abductor indicis of the same arm. Fig. 2 is a typical record. Result—the abductor indicis (*c*) is first in order of time. The average difference .017 sec. M.V. .005; the approximate difference between the abductor indicis and the biceps being .033 sec.

Incidentally, it may be observed that, if the tambour is placed upon the index finger near the end of the second phalanx, instead of upon the muscle itself, the extensor carpi radialis

longior contracts sooner than the movement of the finger. Average difference .02 sec. M.V. .005. Thus the difference in time between the beginning of the contraction of the abductor indicis and the movement of the index finger is approximately .037 sec. This represents the time required for the muscle to shorten sufficiently to pull on the finger. This result was further verified by placing one tambour on the finger and the other on the abductor.

In all of the above experiments special precautions were taken to secure accurate results. The point of contact on the arm was marked with ink, so that repetitions of the test could be made without variation. In order to eliminate possible error in the apparatus the tambours were tested as to readiness of response to pressure and were changed in order at various intervals during the experiments. In all, about 250 time measurements were taken.

From the foregoing it appears that when a motor discharge is directed to the extreme accessory muscle of the arm the law of diffusion is, primarily, to the muscle directly innervated, and, secondarily, to the adjacent related muscles in the order of their distance anatomically from the muscle innervated.

From all this it follows that exercise of the abductor indicis would train the other muscles of the arm, thus strengthening the whole. And then, if exercising the biceps will in turn train the muscles of the forearm and hand, we have evidence of a downward diffusion as well as an upward one. The following experiments were undertaken to test this inference.

The clamping attachment for the hand previously mentioned served for practicing the abductor indicis. In case of the biceps, the arm rested in a support with the forearm at an angle of 45° with the arm, permitting free flexion at the elbow; a small board was strapped to the inner side of the forearm, extending over the palm to the ends of the fingers. Attached to this board just below the wrist was a cord, pulling upon a dynamometer and also operating the recording point upon the kymograph. In this apparatus the forearm and hand became a rigid lever, pivoted upon the elbow and dependent for action upon the muscles of the arm proper.

When the muscles of the forearm were tested the arm was put in the same position as for the biceps, the forearm secured to an immovable board, and a shorter one placed upon the palm, extending from the wrist to the ends of the fingers, secured in such a manner that the only possible movement was flexion of the hand as a whole. A cord connected with the dynamometer as in the case of the biceps.

With this apparatus it was possible to get accurate records of the flexor muscles of the forearm, the biceps and the abductor indicis.

The first experiment was the training of the left biceps. The subjects were *R* and *W*, both young men in good physical condition, throughout the whole series. All records were taken in the laboratory from 8 to 9 a. m. Preliminary tests were made of the right and left abductors, the left forearm and the left biceps. On February 14th exercise of the left biceps was begun and continued fifteen days. The pulls were made in the ordinary counting rhythm. During the first five days 100 pulls were made daily without rest intervals, for the second five days 125 pulls and for the last five days 150 pulls. At the close of the practice period final records were taken of all the muscles previously tested.

Results of Practice.

	Initial Resist. of the dynamometer.		Average Pull in mm.				Gain.	
	W.	R.	W.		R.		W.	R.
	Kilos.	Kilos.	Prelim.	Final.	Prelim.	Final.	%	%
1. Train. left biceps.								
" "	8.2	8.2	41.5	52.3	36.9	53.0	26	43
" forearm.	5.2	5.2	31.0	27.3	38.0	38.3	—	0.8
" abductor.	1.0	0.4	6.0	11.8	11.8	16.5	100	40
right "	1.0	0.4	5.0	6.3	14.0	17.0	26	21
2. Train. r. abd. indicis.								
r. abductor.	1.0	1.0	6.7	17.0	10.7	23.2	153	117
r. forearm.	8.2	8.2	14.0	30.8	21.8	32.7	120	50
r. biceps.	8.2	8.2	44.0	47.4	41.0	54.0	8	32
left biceps.	8.2	8.2	52.3	53.3	53.0	55.4	2	4
left abductor.	1.0	0.4	11.8	18.0	16.5	24.0	52	45

In order to increase the certainty of the results the other arm was practiced in reversed order. At the close of the above

series preliminary tests of the right arm were taken, and practice of the right abductor indicis begun March 8th and continued for fourteen days—100 pulls as before. Final records were taken for the muscles of both arms. The results for both experiments are given in the table.

As the table indicates, the training of the left biceps resulted in a gain of 26% for *W* and 43% for *R*. A defect in the adjustment of the recording apparatus, not discovered soon enough for correction, gave a very doubtful record for the forearm final, so the result in this case must not be set down as a positive exception. The unexercised left abductor shows a decided gain of 100% (*W*) and 40% (*R*). The right abductor made a gain of 26% (*W*) and 21% (*R*). We have here a measure of the reactionary effect of one arm upon the other, as well as that of a fundamental muscle upon an accessory.

The results for the right arm are quite significant. The exercised abductor indicis gained 153% (*W*) and 117% (*R*). The reactionary gain of the forearm muscles (120% for *W* and 50% for *R*), while large, was less than the gain of the abductor itself. This gain cannot be attributed to the direct exercise of the extensor carpi radialis longior, because with this apparatus movements of flexion only were made. The biceps, the most remote anatomatically, gained correspondingly less—8% for *W*. and 32% for *R*. These results are in harmony with all our previous observations.

The records of the left, or resting arm, give the abductor a gain of 52% (*W*) and 45% (*R*) and the biceps 2% (*W*) and 4% (*R*). From this it appears that the accessory muscles of one side gain approximately as much from the exercise of the corresponding muscles of the opposite side as from the exercise of the fundamental muscles of the same side. That is to say, the diffused motor discharge from one side to the other is as great as that from the shoulder center to the finger center. The idea that training one muscle trains others is not a new one, as the various published observations of transference of practice from one side of the body to the other indicate.

It may be objected that the gains of the left arm, attributed to the exercise of the right, may be simply the result of resting

after a period of training; but the left biceps and the right abductor were the only muscles trained. The left abductor had no direct training, and shows a total gain throughout the entire series of about 300%.

In demonstrating diffusion by time relations the muscles were taken in series, but in these experiments this relation was disregarded and the muscles studied in groups, corresponding to the sections of the arm, so that the results are indicative of a more general diffusion than the preceding.

From the above it seems certain that the exercise of any muscle reacts upon all other related muscles, which is to say that diffusion takes place in both inward and outward directions. Though the above quantitative comparison of gains indicates certain relations, yet our ignorance as to what are equivalent gains for different muscles makes it impossible to put forward the following in any other form than mere hypotheses:

a. That the exercise of an accessory muscle has a greater reactionary effect upon the adjacent fundamental muscles than upon the more remote.

b. That an accessory muscle of one arm gains as much from the training of the corresponding muscle of the opposite side as from the training of the fundamental muscle of the same side. In terms of motor discharge this indicates that these centers occupy the same diffusion level.

c. The reactionary, or secondary, gain of the fundamental arm muscles from the exercise of accessory arm muscles is less than when the conditions are reversed—*i. e.*, the fundamental muscles practiced and the accessory reacted upon. This is in harmony with the accepted order of motor development.

It is now time to consider the bearing of these results upon a few important questions. In the first place, it is desirable to know where this diffusion takes place—whether in the cord, the lower centers of the brain, or in the cortex. The fact that the order of diffusion is backward in the arm indicates that it is not due to the branching of the nerve, since in that event it would reach the nearest muscles first. If it is claimed that the impulse originates in all of these centers at the same instant, it can still be answered that the muscles slowest in response are anatom-

ically nearest the cortex. The biceps is innervated through the fifth and sixth cervical nerves, the extensor carpi radialis longior and brevior through the sixth and seventh and the abductor indicis through the eighth. (Quain's Anatomy.) So the muscles slowest in response are both in length of nerve tract and in relation to their innervating centers nearest the cortex. All this tends to show that the discharge starts down the finger tract first when its muscle is to be innervated, or in more general terms the discharge reaches the object of volition first. But it may be answered that we have here simply the differences in the latent times of the muscles, and that until it is shown that their latent times are equal no certain conclusion can be reached. This objection the present data cannot meet. A study of the reaction-times of these muscles may finally solve this problem. So we cannot yet say where this diffusion occurs, since the anatomical arrangement and our present knowledge of cerebral localization make it possible for it to occur either in the cord or in the cortex. Further research in the time relations of the expressive movements of facial muscles, accompanying states of physical effort, may throw some light upon this point. If it does take place in the cord, efferent diffusion is both upward and downward, according to the foregoing data, and not downward only as some physiologists have assumed.

The natural method of determining the order of downward diffusion would be with tambours, as in our first experiments; but this is rather difficult and will require new apparatus, so that no results can be given at this writing.

Our experiments in training were undertaken to show that diffusion took certain directions, but it is well to view the results in relation to previous research in 'cross-education' and transference of practice effects from one side of the body to the other. Two very recent publications by Dr. R. S. Woodworth[1] and Walter M. Davis[2] have so completely summed up the past and so well advanced our present knowledge that we shall discuss our results in relation to them alone.

[1] Accuracy of Voluntary Movement, PSYCHOLOGICAL REVIEW, Supplement, Vol. 3.

[2] Cross-Education, Studies from Yale Psychological Laboratory, Vol. 6.

Dr. Woodworth gave some attention to the transference of accuracy of movement from one side to the other. He says: "These results show (1) that the transference of the effects of practice from one side of the body to the other—a transference which has been established in other investigations as taking place from the right side to the left—also takes place from the left side to the right, and (2) that it is not the mere practice that has this transferred effect, but only successful practice. * * * Where the left hand does gain the right shares the benefits in almost equal measure."

Mr. Davis gave his entire attention to the transference of practice effects. He finds that resistance to fatigue and the size of muscles increase on the unpracticed side. In brief, he finds the following: "(*a*) The effects of exercise may be transferred to a greater or less degree from the parts practiced to other parts of the body. This transference is greatest to symmetrical and closely related parts. (*b*) There is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or in position."

In one part of his work Mr. Davis made experiments similar to our own. He trained the biceps with dumb-bell exercise and recorded the gain in grip, assigning as a cause of the latter the observed clenching of the hand due to the onset of fatigue. Our experiments show that there is an actual contraction of the related muscles of the same side corresponding to the observed practice effects; and, since the practice effect appears upon the other side also, it is natural to infer that those muscles contract. As previously shown, it is possible for a muscle to make an appreciable contraction before movement of the limb occurs. For this reason it must not be claimed that muscles do not receive cross-exercise until more refined methods of observation are used than heretofore in such experiments.

There is a disposition among writers to explain 'cross-education' as a change in the motor centers of the opposite side, resulting from the close functional relation of the two halves of the brain, but this is upon the assumption that the muscles of the opposite side do not contract. Even with such an assumption

it is difficult to see how a change, resulting in a contraction, could be transferred to another cell without resulting in a contraction of the muscles with which this second cell holds functional relations. So far, all results are in harmony with the explanation of the case by diffused motor currents. In our experiments the gains of the muscles receiving the diffused currents were less than the gains of those directly innervated, and in the researches just mentioned the gains of the unpracticed side were less than those of the practiced side. Naturally as the current spreads it gets weaker and weaker. The question can be definitely settled by placing delicate tambours upon the muscles of the opposite side. It would also be interesting to try the practice effect in simply thinking the movement.

On the whole, it seems that any attempt to explain the transference of practice from one part of the body to another must consider the muscle, nerve and center as a unit. Of course, we are speaking of gains in strength and endurance only; gains in precision may hold different relations to the mechanism.

In conclusion this study leads to the belief that there is a diffusion of the motor current in the arm, and that it follows out an order corresponding to anatomical and functional relations such as would occur in an irradiation of the current in the cells of the cortex or in the spinal cord. Also, it seems to follow the lines of anatomical and functional development, the accessory motor tracts being so intimately connected with the fundamentals that there is a constant leakage of the current into these old channels. By further research we hope to reach more specific conclusions.

We wish to express our indebtedness to Dr. J. A. Bergström, Indiana University, for some valuable suggestions in the formative stage of the work and for experience gained from assisting him in some yet unpublished research with the ergograph. The experiments here reported were made in the psychological laboratory at the Ohio State University.

THE COLOR CHANGES OF THE WHITE LIGHT AFTER-IMAGE, CENTRAL AND PERIPHERAL.

BY DR. MARGARET FLOY WASHBURN.

Wells College.

Two problems were investigated in the observations described in this paper. The first concerns the dependence of the series of colors seen in the white light after-image upon the duration and intensity of the stimulus. It is well known that the flight of colors in an image produced by prolonged stimulation with ordinary daylight differs considerably from that occasioned by shorter or by instantaneous stimulation, but the relation of these different color series to each other has not yet been made out. For about a year and a half I have been studying this relation by a method of experiment which I believe to be new: the method, namely, of overlapping two images in such a way that the portion of the retina which corresponds to the overlapped part of the image shall have been stimulated twice as long as the portions corresponding to the rest of the image. The effect of increased duration of the stimulus may be studied in this manner far more conveniently than when the comparison has to be made between successive images.

The second problem is one that, in the form in which it is here investigated, has received little attention from writers on the subject of after-images. It concerns the series of colors to be observed in an image which falls towards the side parts of the retina. The observations here recorded represent the merest beginnings of such a research: they have resulted in the establishment of only a few scattered facts, but they are perhaps worth publishing if they suggest a field of study to workers who have better opportunities for investigation and fuller theoretical knowledge of the subject than the present writer possesses.

I. The Dependence of the Color Series on the Duration and Intensity of the Stimulus.

If one looks steadily at a bright white sky, *i. e.*, a sky covered by a thin uniform cloud, or at a sun-illumined snowfield, for about fifteen seconds, and if the eyes are then closed and covered with the hands, the phenomena observed will be as follows: Just as the eyes close there will be a short after-image of the window or the luminous object; this will usually disappear, and after a few seconds a bluish white image will be seen, deepening to a rather bright light blue. A vivid green color succeeds, lasting a long time, and at length giving place to red. The red image is succeeded by a dark blue image, and this again by a very dark green image, whose color is sometimes scarcely distinguishable. If the image is that of a cross-barred window, the change from the positive to the negative image, as indicated by the brightening of the lines representing the bars, begins at the end of the first green stage, and is complete about the middle of the succeeding red stage.

This series, then, blue, green, red, blue, green, is the color series which occurs for maximum duration of the stimulation, and for the maximum intensity obtainable by ordinary daylight. I shall, therefore, call it the maximum color series. Increasing the duration of the stimulus produces no change in the above sequence of colors. It is identical with that observed by Helmholtz and Fechner under the same conditions. The blue and green of the final stages are darker than the earlier blue and green, which are also less saturated, suggesting that the latter are accompanied by a brightness process which diminishes in intensity as the image runs its course: otherwise, the color series looks like a circular one, and indicates that if the image lasted long enough, we should have a second red stage, completing a second cycle of blue, green, red.

Now if it happens that the field of stimulating light is of different intensity or brightness at different parts; if, for instance, we have an irregularly clouded sky, we find, as Helmholtz observed in the after-image of the sun's disk, that the more intensely stimulated parts of the image take longer to go through

this succession of colors than the less intensely stimulated parts. When the latter have passed into the first green stage the parts corresponding to brighter spots in the stimulating field persist as spots of blue; these spots remain green after the rest of the image has turned red, and so on, persisting in each color stage after the surrounding parts of the image have entered the succeeding stage. Thus we see that after the maximal series has been reached increase in the intensity of the stimulating light increases the duration of the several color stages in the series.

Now by the method of overlapping images it can be shown that increasing the duration of the action of the stimulating light has the same effect as increasing its intensity. The overlapping of the images can be very easily done by first fixating one point on the bright surface for, say, fifteen seconds, closing the eyes for an instant, and then fixating a different point on the stimulating surface for fifteen seconds. Two images will then be found, which will partially coincide or overlap; and the portion of the retina corresponding to the overlapped part of the image will have been subjected to a stimulus of twice the duration of that which has affected the parts covered by the non-coincident portions of the two images. Now when the brightness of the light is such as to give the maximal color series after a fifteen-second exposure, the overlapped parts of the two images, which have been subjected to a stimulus of thirty seconds' duration, behave just like the images produced by intenser stimulation. That is, when the other parts of the two images have passed into the first green stage, the overlapped part remains blue; when they have become red it remains green, and so on; in other words, when the maximal series has been reached, further increase in the duration of the stimulus increases the duration of the color stages in the series.

Now when the intensity of the illuminating light is less than we have supposed it to be in the experiments just described; when, for instance, the sky is rather heavily clouded; or when the duration of the stimulus is reduced to seven or eight seconds, a different series of colors is observed. The image is at first blue; then it passes through a momentary nearly colorless stage into red; this is followed by the darker blue stage and this again

by a very faintly greenish black. The characteristic difference between this series and the maximal series is that the first green stage is dropped out: the image passing either directly or through a very short period of colorlessness from blue into red. This fact also was noted by Helmholtz. If certain parts of the stimulating field are brighter than the rest the corresponding parts of the image pass through the green stage, while the surrounding portions omit it. And if two images be overlapped as described above, the overlapped parts show the green, while the other parts do not. The conclusion to which these phenomena lead is that the first qualitative effect of diminishing the intensity or duration of the stimulus is the dropping out of the first green stage in the color series. The total duration of the image is also, of course, shorter.

In the next place, if the intensity or duration of the stimulus be still further diminished; if, for instance, the sky be looked at for not more than five seconds, the sequence of colors undergoes another change. The image is blue or violet at first, passes quickly from violet to red, and from red into a greenish black. Between the red and this final stage there is often noticeable a stage of washed out or indefinite color, which Helmholtz calls 'dull orange,' but which is not unlike the colorless phase mentioned above as taking the place of the first green image. The violet seems to be due to the rapid transition from blue into red. Now when two images of this sort are overlapped in the usual manner, the overlapped parts show the second blue stage, which does not appear on the remaining portions. It is evident that the next qualitative effect of diminishing the intensity or duration of the stimulus is the dropping out of the second blue stage from the color series.

The progressive effect of diminishing intensity or duration might then be stated as follows: Shortening the stages of the maximal series (blue, green, red, blue, green), dropping out the first green stage; shortening the stages of this second series (blue, red, blue, green), dropping out the second blue stage.

My observations show that the minimum of intensity and duration which will give a colored image at all, results in a violet-red image followed by a greenish-black image, the violet-

red seeming to be the effect of the rapid transition from blue to red. It must be borne in mind that these experiments show that intensity and duration of stimulus have precisely the same significance for the color changes of the image: that is, a thirty-second exposure to moderate light produces the same effect as a fifteen-second exposure to stronger light. Thus when we find in Helmholtz or elsewhere, a statement of the color series resulting from 'instantaneous illumination' we must know the intensity of the illumination before we can understand the significance of the series. A stimulating field that, like the sun's disk, gives different brightness at different parts may yield results of great complexity, such as those described in a footnote by Professor Külpe (*Outlines*, p. 131); but my own observations show these results to be in accord with the general laws above stated. Practice will enable one to distinguish, from the colors of the image at a given instant, which parts of it were produced by intenser stimulation and which by duller light.

To present a theory accounting for the observations described above is not possible, at least for the present writer, while the physiological significance of the flight of colors is so imperfectly comprehended. One or two theoretical points do, however, seem to be fairly deducible from these facts. First, the fact that the colors in the earlier stages of the image are so much 'lighter' and less saturated than those in the later stages suggests that the color processes of the image are accompanied at first by a colorless process, which gradually diminishes in intensity as the image runs its course. Second, if we adopt the ordinary theory that the series of colors is due to the fact that the various color processes set up by the action of white light on the retina differ in the rapidity with which they lose intensity on the one hand and the length of time during which they persist on the other, then we must suppose that the degree to which color processes differ in these respects is a function of the intensity of the stimulating light. For instance, if the intensity is diminished, the difference between the green process and the blue or red process is abolished and the image passes from blue into red without exhibiting an intermediate stage. Such a supposition meets with many difficulties. But, thirdly, it is hard to reconcile the current

theory, as stated above, with the observation that the series of colors in the image is a series that tends to repeat itself. Blue, green, red, blue, green, is the full color sequence, and the later blue and green stages differ from the earlier, to all appearance, only in intensity and saturation. Now, why, if the red color is due to the fact that the other color processes have been exhausted, should it be followed by a revival of the exhausted processes?

II. The Color Changes of the Peripheral White Light Image.

Aubert, I believe, made the most systematic observations on record of after-images produced on the side parts of the retina (*Physiologie d. Netzhaut*, pp. 375 ff.). The white light stimuli which he used were electric sparks, and he describes the image as invariably colorless even within the zones of possible color stimulation. It occasionally looked larger than an image seen in direct vision. This fact he thinks due to imperfect focussing of the image. Even when colored glass was interposed before the stimulating light the image was colorless. Aubert also experimented with the negative images of small squares of colored paper seen indirectly. He found that the negative images had the color which they would have had in direct vision, and that they were of shorter duration than central or foveal images. As to their relative size compared with direct vision images he says nothing.

The experiments whose results are discussed below do not involve accurate quantitative determinations, exact measurements not being possible with the apparatus at hand; and the few facts gathered from them may be easily verified by any observer. The light used was white daylight, of an intensity and duration sufficient to give the maximal color series in a central image. The width of the image, that of a square opening in a black window screen, corresponded to about 1½° on the retina. It was observed at distances from the fovea of between 5° and 15°.

1. The first noteworthy observation is that the peripheral image is smaller than the central image. This is readily seen

if light be admitted into a darkened room through two openings of the same size, the observer fixating one of these openings. The after-image of the indirectly seen opening is noticeably smaller and narrower than the image on the center of the retina. Why this should be so becomes evident from a study of the diagram showing the projection of the Helmholtz Circles of Direction.

2. The positive white-light image which is located outside of the macula lutea is practically colorless in the darkened field of the closed and covered eye. Sometimes a faint green color is distinguishable, but, in general, I have found Aubert's statement as to the colorlessness of the image confirmed.

3. The negative image of a white light, indirectly seen, is as Helmholtz says, very difficult to observe *in the darkened field.* Usually a long interval occurs between the positive and negative images. But when it comes it is always *colored*, a violet red, often scarcely distinguishable from the dark field, but unmistakable in color when seen. This, again, is consistent with Aubert's observation that the negative images of colored papers are distinctly colored on the side parts of the retina.

The foregoing observations were all made in the darkened field of closed and covered eyes. If the peripheral image be examined when light is let in through the closed lids, two curious and interesting phenomena occur. These may be studied best when simultaneous peripheral and central images are produced by the method suggested above.

4. If the two images be watched in the darkened field, the central image will have the usual color, and the side image will be colorless. But if light be admitted by removing the hands from the closed eyelids, the side image will flash out into a peculiar brilliant green, like a bit of stained glass, and giving the same transparently luminous effect. The foveal image does not seem to give this phenomenon at all with ordinary intensities, but a similar appearance may be observed in the foveal image of the sun's disk. The fact that the green color is seen by transmitted light suggests that it is a fatigue phenomenon, and one that appears in peripheral images more readily, *i. e.*, at a lower intensity of the stimulating light, than in central images.

5. The second observation to be made when light is admitted into the field of vision concerns the duration of the effect of fatigue. When peripheral and central images are produced simultaneously in the manner described above and studied in the darkened field, the side image disappears before the central one; as Aubert says, the peripheral image is of shorter duration. The difficulty of keeping the attention fixed on a faint object towards the side of the field complicates the observation here. However, after both images are practically lost in the darkened field, if light be admitted through the lids or by opening the eyes, the peripheral negative image is visible as a black spot long after all trace of the central image has gone. Nothing could be more strikingly evident than this phenomenon. I have found the difference in duration to be as great as five minutes, and have no doubt that it was often more. The colored image seen in the darkened field, which I have above called a negative image, disappears sooner if it falls towards the side regions of the retina. But the black image seen when light enters the field lasts longer when it is situated towards the periphery. And this image is necessarily and unequivocally a fatigue phenomenon. The conclusion suggested is that the effects of fatigue are more lasting towards the side portions of the retina than near the center.

One more comment remains to be made on the results of these experiments. As has been already said, the unsaturated character of the colors in the early stages of the foveal image suggests that an achromatic or brightness process accompanies the color processes at first, becoming less and less intense during the course of the image. May not this supposition explain the fact that the early stages of the side image are colorless or very nearly so (Aubert says 'faint yellowish'; I should call the color greenish, where any color can be seen), while the later stage is distinctly colored violet red? The ratio of the colorless process to the color process would naturally be greater towards the side of the retina, and during the first stages of the image the color process might be wholly swamped. Later, when the achromatic process had diminished in intensity, the color stage would become visible.

THE PSEUDOSCOPE AND SOME OF ITS RECENT IMPROVEMENTS.

BY PROFESSOR JOSEPH JASTROW.

University of Wisconsin.

The pseudoscope forms the complement of the stereoscope; the latter demonstrates the significance of the several factors in the perception of the third dimension of space; the former completes the demonstration by showing that the interpretation of positions in the third dimension may be inverted by interchanging the points of view of the two retinæ. In both cases the success of the demonstration depends upon the suitability of the instrument, as regards convenience, precision, accuracy of adjustment to different eyes, and the naturalness of the viewing process. The selection of the object viewed is also an influential factor. There are thus many kinds and degrees of stereoscopic as of pseudoscopic effects. In another connection (*Science*, May 6, 1898), I have reviewed various forms of the stereoscope and of stereoscopic accessories which are of service to the psychologist in the demonstration and explanation of this most important group of psychological inferences. I desire now to notice in a similar way some inventions and devices in the field of inverted stereoscopy, or pseudoscopy, if the term be allowed.

By far the most important of such inventions is the new form of pseudoscope, designed by Professor R. W. Wood, of the University of Wisconsin, and described in *Science*, November 3, 1899. The accompanying cuts, Figs. 1 and 2, illustrate the form and principle of the apparatus. Its essential parts are a pair of stereoscopic prismatic lenses, mounted just as in the stereoscope, and a pair of ordinary double convex lenses held at a suitable distance in front of these.

The inverted and reversed images of the object viewed, formed by the outer pair of lenses, are in turn combined (and

enlarged) by the stereoscopic lenses. Professor Wood thus illustrates this principle: "Let A and B be two points in space, B being in front of A. An eye at X will see B to the right of A, and an eye at X' will see B to the left of A, and the fusion

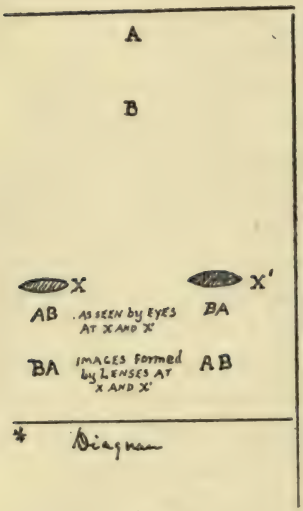


FIG. 1.

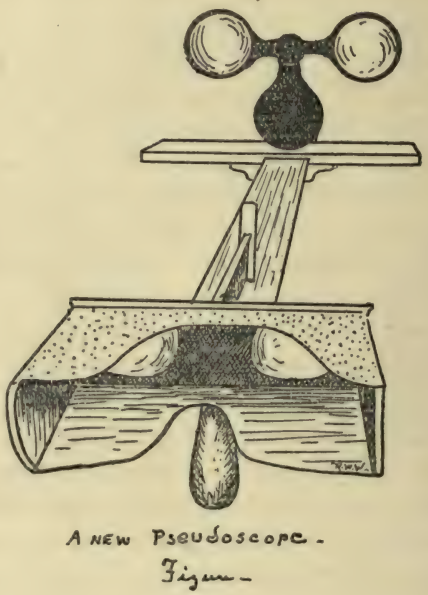


FIG. 2.

of these two images produces stereoscopic vision, B appearing nearer than A. Suppose now that X and X' represent the two lenses. The images which they will form in space will be reversed [and inverted, although for the sake of simplicity they are drawn erect], that is, the lens X will give an image in which B will be to the left of A, or just the opposite of the appearance presented when the eye is at X. It is apparent that the images BA and AB formed by the lenses are identical with what would be seen by eyes at X and X', provided A were in front of B, consequently the fusion of these two images makes A appear nearer than B."

To perfect the instrument, especially as regards convenience of use, the outer pair of convex lenses may be set in tubes at

the outer end of which are the stereoscopic lenses; the one pair of tubes slides within the other, thus permitting the distance between the two lenses on each side to be varied. The apparatus thus takes the form of a small double telescope, or an elongated opera-glass, and is now being constructed in this form.[1] It should not be lost sight of that this method of securing the pseudoscopic effect is entirely different from any other.

In practice, the Wood pseudoscope gives more striking results than any which I have used, and promises to become the standard form of the instrument. It accomplishes the inversion of suitable objects with a vivid likeness to reality that is startling. The objects may be small or moderately large, and in any ordinary position and at a moderately variable distance; it may be used much like an opera-glass, except that it is limited to objects at fairly close range. As to the objects, simple geometrical shapes with rounded contours are to be preferred; bowls, glasses, turned forms with depressions and elevations, mathematical models, and so on, may be recommended. But complex forms are, by no means, excluded; even such as a typewriter or the view of the miscellaneous objects in a room or a street view. But in these a larger number of opposing factors enter, and the desired results cannot always be secured. The inversion suggested by the interchange of retinal relations is opposed by the conflicting evidence of light and shade, of the interposition of objects, of the unnaturalness of the entire result. Thus it is difficult to see pseudoscopically the human face: here apperception constitutes an influential factor, and when the suggested inference goes against the grain of experience, the suggestion fails or is not completely realized. But the observer may notice some effect of this rivalry of motives, to use Professor Stratton's apt phrase, the pseudoscopic view 'saps the life' out of the true perspective, even when it does not replace the latter. The variety of interesting pseudoscopic effects—the manifold transformations of cameo to intaglio, of foreground to background, of advancing contours to receding ones—has not been sufficiently appreciated, and mainly because of the

[1] By the Chicago Laboratory Supply Co., who will Probably be prepared to furnish the apparatus.

rather unsatisfactory kind or limited range of such effects which the ordinary forms of pseudoscope have yielded. To a large degree, these disadvantages have been overcome by this new form of the instrument.[1]

In this REVIEW for November, 1898, Professor Stratton describes a mirror pseudoscope of novel design and valuable alike for its illustration of the pseudoscopic principle, and for its variability of application and consequent aid in the analysis of the component factors of the pseudoscopic effect. One of these variations allows of an exaggerated pseudoscopic and another of an exaggerated stereoscopic effect, both of which advantages

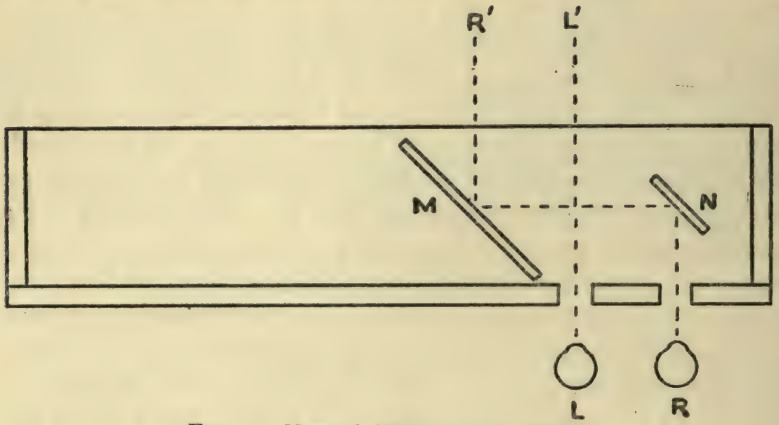


FIG. 3. Normal Pseudoscopic Vision.

are possessed only by the Stratton pseudoscope. Professor Stratton has given an able description of the nature and applications of this instrument, which may be referred to for further details. The accompanying cut (Fig. 3) illustrates the principle by which the right eye obtains a 'left-hand' view and the left eye a 'right-hand' view of the object to be seen pseudoscopically.

[1] Although the announcement is somewhat premature, I am tempted to add a word regarding an application of the Wood pseudoscope, which I am now contemplating. I am studying the capacity to judge of the relative convexity of a series of convex forms of variable convexity, viewed under a uniform shadowless light. It would be difficult to apply the test to a similar series of concave forms because the receding parts of the concave forms would be in relative shadow; but by viewing the convex forms with a pseudoscope the desired result is attained most readily. An account of these experiments will be published in due season.

I cannot infer with certainty from Professor Stratton's description, whether he regards that the mirror form of pseudoscope is itself novel or only his form of it. At all events, there is a mirror pseudoscope designed by Professor Ewald, and made by Major, of Strassburg, and the Laboratory of the University of Wisconsin possesses such an apparatus. The setting of the mirror and the path of the rays by which the 'left-hand' view of the object reaches the right eye and the 'right-hand' view of the object reaches the left eye can be readily understood by reference to the accompanying figure (Fig. 4). In practice the instrument is extremely limited in scope. The objects to be viewed must be small and practically at one distance only; and even then only a measurably satisfactory effect is obtained, by no means comparable in life-likeness to that of the Wood pseudoscope. Professor Stratton's arrangement of the mirrors is in every way more advantageous and makes the mirror pseudoscope a valuable form for many purposes.

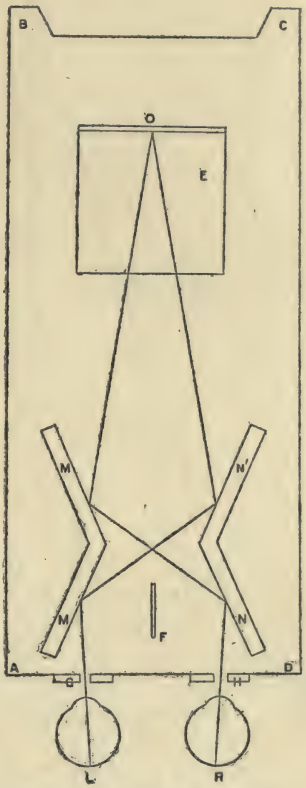


FIG. 4. *A BC D*, the board on which the parts are mounted. *E*, a mirror and screen to regulate the light. *F*, a screen to prevent the eye from seeing both mirrors. *G* and *H*, openings for *L* and *R*, the left and right eyes. *O* is the object viewed.

The inversion by total reflection in prisms which was the original form of the Wheatstone pseudoscope, has thus been added to by (*a*) inversion by reflection in mirrors, and (*b*) by inversion by fusion of the inverted images of convex lenses. To complete the review of the pseudoscopic method I give the principle of the Wheatstone total reflection pseudoscope from Sanford's Experimental Psychology, pp. 280–281. (See Fig. 5.)

One other form of pseudoscopic effect remains to be noticed. namely, the familiar one of interchanging the pictures of the ordinary double stereoscopic photograph. The most suitable objects are geometrical diagrams, and of these by far the best is the series of mathematical curves of motion, photographically produced by Professor Sclichter and referred to in my article (*Science*, May 6, 1898). The most satisfactory method of securing the inverted effect is by use of the modification of the 'perspectoscope,' which I described in the same article. I have modified the instrument by pivoting the mirrors instead of retaining them in fixed positions. Setting the mirrors so that when viewing the ordinary stereoscopic card, I see the mathematical curve or photographic picture in true perspective (*i. e.*, the right eye sees the right-hand picture and the left eye the left-hand picture), I have only to alter slightly the angles of the mirrors so that the reflection of the left-hand picture is seen by the right eye and that of the right-hand picture is seen by the left eye, and the reversal of perspective is at once accomplished (Fig. 6). Not only can the same card be used for stereoscopic and pseudoscopic purposes by means of this instrument, but one may pass to and fro from one to the other effect at will and with but a slight movement of the mirrors. I am now arranging such an apparatus so that the mirrors naturally stop at the two positions desired.

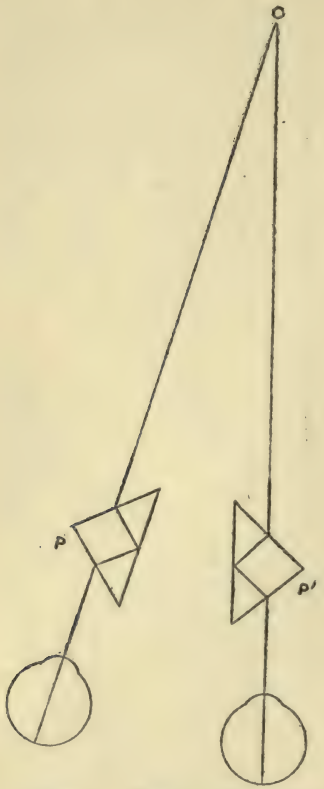


FIG. 5. *R* and *L*, the right and left eyes. *P* and *P'*, the prism. *O*, the object. "The hypothenuse of the prisms practically acts as a mirror, reversing the view of the object; the direction of the ray is unchanged. Each eye thus sees a reversed view of the object."

In instruction in psychology, the analysis of the factors of

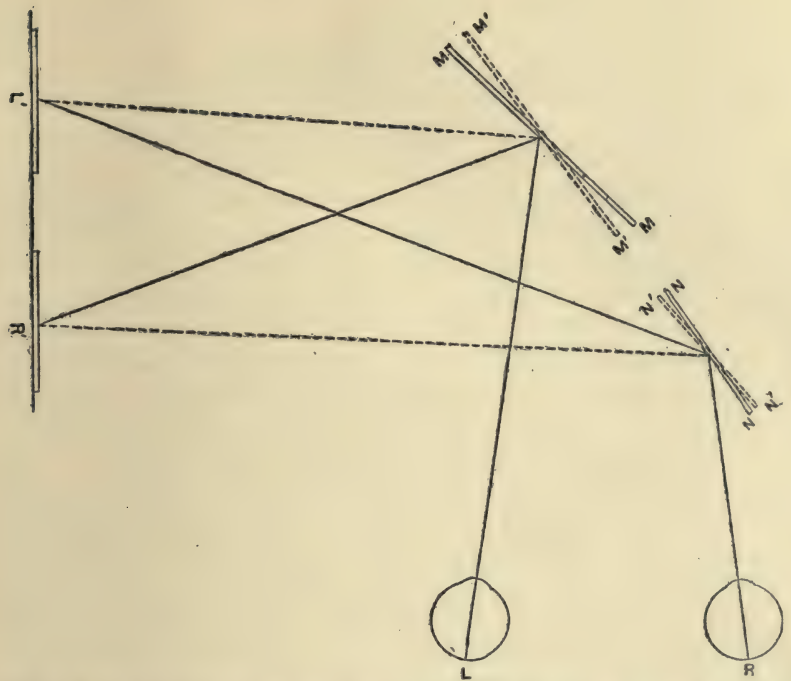


FIG. 6. R and L, the right and left eyes; R' and L', the centers of the right and left halves of an ordinary stereoscopic card; MM and NN the mirrors pivoting on their centers. When the mirrors are in the positions of $M'M'$ and $N'N'$ R sees R' and L sees L', as indicated by the dotted lines, and the ordinary stereoscopic effect results. When the mirrors are set in the positions MM and NN, R sees L' and L sees R', as indicated by the dotted lines, and there is a pseudoscopic effect.

depth-perception is so helpful to a right understanding of the nature of a 'perception-complex,' and furnishes so useful an illustration of many psychological principles, that all devices that contribute to the clearer and more convenient demonstration of the several elements of the complex, should find a welcome place in the psychological laboratory.

DISCUSSION AND REPORTS.

PROFESSOR STUMPF ON EMOTION.

Stumpf's attractively written and well-constructed article, 'Ueber den Begriff der Gemüthsbewegung,' a notice of which appeared in the November number of this REVIEW, deserves, I think, some further discussion; for its tendency, as it seems to me, is clearly to bring together the two main lines of divergence in the controversy on the theory of emotion. Stumpf himself, to be sure, undertakes to defend the common doctrine as over against the 'sensualistic' theories of Ribot, James and Lange; but in the end he makes such large concessions to these theories that we are enabled at once to judge of the extent of their influence in modifying the traditional conceptions, and also to see more clearly what is yet needed in order to bring about something like substantial agreement. Stumpf defines an emotion (*Affect* = *Gemüthsbewegung*) as 'a passive state of feeling relating itself to a judged situation' (*Sachverhalt*). He considers the judgment—the term taken in the widest sense—essential, not merely as a condition, but as an integral part of the emotion itself. Assuming that the term 'passive' is intended not to exclude all motor tendency, but merely to mark off in a practical way the emotions as such from the impulses as such, I should myself, though holding in general to the 'sensualistic' hypothesis, heartily accept this definition. As to the further statement which makes the judgment a constituent part of the emotion, more will be said later. Stumpf criticises Ribot for identifying emotions with sensation-feelings; he criticises James and Lange for refusing to recognize an affect-process distinct from the sensory report of muscular and organic disturbance. In the extended criticism of the James-Lange hypothesis which occupies the larger part of the article, there is, of course, much repeated that has been said already, and at some points the criticism, as usual, would seem to rest on misunderstanding. In reply, for example, to the argument that if we abstract in our thought of a strong emotion from its organic 'accompaniment,' nothing of the emotion is left, we are told that the chief ingredient in our thought of an emotion is not organic phenomena at all, but visual imagery, *e.g.*, red or pale cheeks,—as if James in this

famous argument were referring to our representation of another's emotion rather than our own! But there is very effective criticism of the pathological cases usually appealed to, especially of Sollier's, and along with this a very proper refusal, I think, to recognize as an emotion in the strict sense any feeling which is objectless. There is also a noteworthy criticism of the so-called law of dynamogeny, regarded as affording a general support for James' theory. The criticism is specially directed against Féré, whose well-known results are strongly suspected and on the best showing regarded as inconclusive for the establishment of a general law. The 'motor' discharges, it is urged, need not pass to the periphery at all, but may remain latent as tensions in the brain till released by new stimuli, or if they do pass to the periphery they may be too weak to be felt. The insistence here, of course, is on the law of the threshold. Unfortunately, this criticism too, interesting as it is in itself, hardly affects James' theory; for although James does hold that every idea is motor in its consequence, he does not claim that every idea necessarily arouses an emotion. Nor do I find specially convincing Stumpf's restatement of the direct arguments against the theory, namely the failure of emotions to correspond in quality, intensity and time-sequence to the bodily changes said to constitute them. It is hard, for example, to believe that the 'tiefe Rührung' inspired by a deed of heroism or the 'Ergriffenheit' of a highly sensitive nature in presence of an artistic masterpiece bears, as Stumpf asserts, no correspondence whatever to increase of pulse, dilation of blood-vessels, etc. It seems to me that we have in this field still little more than dogmatic assertions on both sides. We need more experiment. But even experimental results will be of little avail unless we know how to interpret them. And here the difficulties are great, not to say insuperable. When, for example, we seek to determine the relation of the intensity of the emotion to its expression, what precisely do we mean by emotional strength? One popular measure of the strength of an emotion is its persistence. But here we tend to pass from the emotion as a conscious state to the emotional disposition, the organized capacity for specific feeling. Evidently it is not this, but only some actual passing phase of the emotion which we can attempt to measure. But how can we determine anything about the intensity of even that? Will our subjective appreciation of its intensity suffice? Then we might, indeed, expect to find very frequently a decided lack of correspondence between the intensity of the emotion so measured and the evidence of our kymographic tracings, for our subjective conviction of the intensity of an emotion is measured on com-

plex standards of value which no actual physical measurements can ever possibly reach. But would this prove the latter altogether fallacious? The question is a highly debatable one; yet surely there must be something in consciousness corresponding to these physical magnitudes, and how far that something is to be regarded as emotional will depend on the general results of psychological analysis and on general theory as to how an emotion is constituted. In principle, at any rate, the difficulty of measurement is no greater here than in the simplest cases of psycho-physical experiment: we obtain in them a measure, if not the sole measure, of psychical excitement, and control the differences of subjective appreciation by allowing something to the method of averages.

But the significance of Stumpf's article, as it seems to me, lies in its recognition of the value of the 'sensualistic' theories and in the concessions made to them. It recognizes their value, namely, in emphasizing the organic sensations in the description, as distinguished from the definition, of the emotions, in pointing to new classifications of them, and in making clearer the conditions of their genesis. And it concedes not merely that the affect-process may in some cases follow, instead of preceding or accompanying, the organic discharges, but that organic sensations are part-contents, and not bare accompaniments of the emotions, whose quality they profoundly affect. Are we not here appreciably nearer to a common understanding? The first thing to be recognized is that James' 'theory' has nothing whatever to do with the logical definition of an emotion, but with its psychological analysis. James' question, What *is* an Emotion? meant, as the whole course of the discussion shows, not, how would you abstractly define it, but what is the content discoverable in your consciousness when you experience it. In other words, the question related to the nature of the experience when examined from the psychological point of view as a content of consciousness. From this point of view definition is description. And surely the psychologist, at any rate, must recognize that the determination of the '*Begriff* der Gemüthsbewegungen,' however important in itself, is after all but a preliminary to the analysis of the experience regarded as falling under the conception. Now Stumpf admits that organic sensations are part-contents of an emotion and commends the 'sensualistic' theories for forcing us to assign to them a much larger part in constituting the experience than is usually done. He further recognizes, as we have seen, that judgment of the relation of the feeling to an object is also an integral part of the experience. He is far, therefore,

from agreeing with Dr. Irons, for example, in thinking that the emotional experience is psychologically unanalyzable. Assuming then that the question is one of psychological analysis, we seem to have here a distinct approach to an agreement. The organic sensations are admittedly part of the emotion; are they not perhaps the dominant and determining part? There are two further questions. The first is this, Is Stumpf right in making the judgment part of the emotion? It would be better, I think, to say, the feeling of the relation to the object. This feeling I hold is always present, qualifying the feeling arising from organic disturbance, so much so, indeed, that when the latter is isolated and regarded simply as a sum of sensations its distinctively emotional character disappears. This may reasonably be conceded. And this brings us to the second question, Is Stumpf's criticism sound, that the James-Lange theory refuses to recognize a distinct affect-process? If what has just been said is true, then we must admit that it is. The distinctive character of the emotional process as such lies precisely in this feeling of the relation of our state to an object. Consciousness is fundamentally feeling; all changes of consciousness are changes of feeling and in emotion proper the relation to the object is clearly felt. Hence, as has often been pointed out, when attention is specially directed to the bodily sensations present in the actual emotional experience the felt organic relation of the elements of our analysis is destroyed, the emotion tends to evaporate and the sensations stand out clear and distinct for themselves. The organic relation of elements in the emotion has its bodily condition, possibly in some associative central function, but it is no doubt there.

The James-theory, so-called, is really a theory and an analysis. Its analysis is imperfect, as Stumpf and others have shown; but it is at least interesting to see so distinguished a critic acknowledging in effect how nearly it comes to being right. The theory that the reaction consisting in bodily discharges, the feeling of which is essential to the constitution of the emotion, is instinctive, has never, I believe, been successfully assailed, though the only attempt to appreciate it, so far as I know, is in the penetrating articles of Dewey, which appeared in these pages several years ago.

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THE GENESIS OF GENERAL IDEAS FROM GROUP PERCEPTION.

By group perception we, of course, do not mean aggregate perception. Thus, if my first experience with oranges is in seeing a dozen on a table, I apprehend these as a group, and not as an aggregate. Mere aggregate vision—and so multiple image in re-presentation which is plainly not general idea—may occur with animals having multiple eyes; but in ordinary single vision the group is grasped together as such, and so in re-presentation the single image embraces the many like things, giving thus a general idea. The empiric germ of the general idea lies then in apprehension of group of several or many; and this group is a common circumstance in actuality, and a necessary one to be grasped by successful adaptation in the struggle of existence. That a mere aggregate of pictures may imperfectly fulfil this function with multiple eyed animals seems likely, but in higher forms single image vision achieves a picture of aggregates in practically simultaneous perception, for example, a number of yellowish roundish objects—oranges—here is real presentative concept of group of the several made in a single focus, and also by the panoramic sweep of the eye which implies use of after-image in the construction of the group. In this way if we first see negroes in a group, we note them as 'blacks,' and refer after-experiences to group 'blacks.' Here by direct inspection objects are seen together as a group, and if next day I see a negro the presentative concept of the previous day is called up in re-representation, and so identifying the negro as 'one of those.' In this interpretation I surely have so far a general idea, and that purely empiric, and which may be considered as the simplest and original form. The practically simultaneous presentation of the individuals as group of very obvious similars—say negroes or oranges—and the use of the same in re-presentation, are the basis of the latter phase where, through reflection and comparison, a number of more or less dischronous presentations recalled are united as group or class, and still later of the self-developing experience in abstract ideation.

Of course group perception in re-presentation is only as yet nascent concept, only general idea in a very crude agglutinative form. The compound group reference is a clumsy and slow method; and so as evolution demands economy the group reference is abbreviated and amalgamated, and the group concept fades into the single image concept, the vague and typical ideal one, representative or vicarious (Wundt) for the many. Thus the idea answering to the word orange

is not a group idea, but a single image so generalized for color, size, etc., that various oranges of different shades and sizes are readily identified under it, whereas in multiple-group-idea the connection is made with the whole group. Hence concept begins with 'oranges' and ends in 'orange.' That by continual reference to the empiric basis as group perceived there tends to come an abbreviation in the number perceived, and a single typical one constructed from the group, is evident if you refer to some experience where you first saw some kind of objects as group, say telephones at an exposition. There you gain in a moment a group perception, which comes up in re-presentation when you afterwards see telephones, but which is continually reduced and made indefinite in later experiences as a datum by which you interpret. Of course many active minds will form the single image concept on the spot, 'anything so and so is a telephone'; but yet the group method may often be traced as evolving into true general idea.

The history of knowledge then is in brief this: the presentation of the individual and recognition by the re-presentation, and practically correlative with this the presentation of masses of individuals and recognition of component members by the re-presentation, which gradually evolves into the vague hypothetical general idea as a shorthand method, which in turn is denoted by language and becomes the basis of all high and self-developing knowledge. The general idea is then a mode of interpretation and as such finite; an omniscient knowledge would know the thing completely without either percept or concept as means, immediate apprehension giving the totality in all its significance. Hence the idea is neither infinite nor eternal. The idea is entirely an empiric help, a mode of reference to the thing through the typical characteristic ideal thing as abstracted from group perception, by which, without the trouble and danger of direct experience, we know the thing for all its qualities for possible experience. And thus as to the distinctive quality of the general idea, we cannot agree with Professor James (Psychology, V. 1, p. 468) that this consists in eternal sameness. Self-identity and dogmatism are plainly the inner characteristic of all cognition psychoses, whether of the particular or general, whether mere apprehension or complete interpretation. That the idealist affirms some or all ideas as absolute is an objective value upon which psychology is not called to pronounce.

So, also, it need not be enlarged upon that the distinction of the one and the many is not a peculiar quality of the general idea, nor is that of mere commonness or generality. The dog seeing the man

unclothed and variously clothed undoubtedly forms some general idea of the manifold yet single individual, just as the child does, just as we do. We perceive, equally, the common content quality as equally making the man and men. Thus both percept and concept imply grasp, and the quality of both is the fundamental unitary aspect of all cognition. And so both the individual and the general idea have the same formula:

Anything which has such and such marks is *the* food or *a* food.

This has such marks.

This is *the* food or *a* food.

All cognition as interpretation by sign must assume this form and all consciousness as containing mediating links—one consciousness as leading from a former to a later one—is implicitly rational; and this link as actively sought in object and consciously used in the struggle of existence is reason as either particular or general idea.

If the general idea is an abbreviation of what we have called group perception, then, since group perception is certainly quite early in the history of life, it is probable that the general idea is early also. The dog sees the pieces of meat, his breakfast, as a whole group; and it seems likely that with constant use this *datum* as representation consisting of complex manifold images should be abbreviated to representative single image. While we cannot attain direct test in the case of the dog or other animals, yet the struggle of existence would seem to require that the clumsy and large compound group of images should quite rapidly merge into a single image which should embody the common qualities of all the multiform individuals of the group perception. The analogous stages of perception as leading to particlar and general idea may thus be enumerated: 1. The one sense with the one object, the simplest percept, as apprehension of the smooth apple by touch. 2. The correlating the senses in immediate apprehension, as in touch, taste, smell, sight of the apple, thus constituting the individual apple by complex psychosis. 3. Direct recognition in representation of the simple and complex individual. 4. Recognition of the essential individuality by the abbreviated re-presentation. Similarly we have for the general idea the four stages, group perception by one sense, by several senses in conjunction, recognition by the complete re-presentation, recognition by the abbreviated re-presentation. It is plain that a new kind of thing, *e. g.*, apple, is as likely first to be given to experience in group as in the single individual, and hence that the phases of development issuing in particular idea and general idea are really parallel, that the general idea is on the same plane as

the particular idea, and not higher, as is commonly thought. The pictures of both individual and group become by the same process highly generalized into an object where certain essential qualities are roughly sketched in abbreviated form, becoming at last a mere notation and sign, of which the evolution of the alphabet from pictographs is both an example and an illustration. The complex simultaneous group perception is certainly more simple and primitive basis for the general idea than the composite photography of objects perceived at different times, which has its place as later and more advanced method. It is plain that the cognition and recognition of the multiple in environment are called for quite early in the struggle of existence. That is, real grasp in perception, in the re-presentation and in its abridgment as general idea may occur among unisensual animals. The paramecium in Mr. Jenning's interesting study (*Am. Journal of Psychology*, x., p. 507) in its feeding contact with 'loose fibrous bodies' may have multiple perception and 'know a good thing when it has it'; and in its continuous feeding, appreciating its food as such, it connects past moment with present by rational interpretation. Again, does not (p. 510) the random *seeking* for food imply ideation as tactile re-presentation? At least the activities of the paramecium suggest awareness, effort and pleasure-pain, if not particular or general representation. But with the dog there certainly seems to be recognition of his master by particular idea, and of men as men by group re-presentation, which probably in such a common case becomes abbreviated into general idea. However, since we have at present no well recognized tests as to consciousness in the lower animals it is unprofitable to discuss the matter, save to remark that the evolutionary doctrine of psychosis points toward an early development of gross general idea as derived from group perception.

However late or early a genesis we find for the general idea, it, on the evolutionary doctrine of the struggle of existence, must like other advantageous life factors originate in severe effortful activity in the critical moment. The one who makes the quickest reference for identification will be the most successful, and at some point in the history of life some organism has, by supreme effort, attained vital advantage by summarizing the group re-presentation in interpreting the present crisis. And if we turn from this theoretical point of view and examine the origin of the ideas in ourselves—the only direct evidence we can have—we find that thought does not arise spontaneously, but the idea has its birth pangs. If we reflect upon our experience in getting the idea of new things, as trolley car or telephone, we perceive that

even in these simple cases the idea is accomplished in self-activity somewhat effortful. And thought is for the vast majority the most laborious and disagreeable of tasks, and with very few who are thoughtful by habit or nature we must refer the origin of habit and nature to integrated effort. We thus cannot agree with those who find the genesis of the general idea in the spontaneous fusion of images into a composite or generic image. And it has not yet been shown how a perception of resemblance results from coalescence of resembling perceptions. This, indeed, gives not even a 'one of those,' but a blurred one which stands merely for and by itself, whose representative value is not understood. However the germ to higher development lies in the simple identification as 'one' (abstraction) 'of those' (generalization). 'That is that,' and 'that is one of those,' give in very abstract language the process of the primitive particular and general idea.

We cannot then agree with Romanes in his doctrine of 'recepts' as the origins of ideas. If we trace the genesis of these recepts we find it in active association, relating or thought. Thus the city man (Mental Evolution in Man, p. 50) who in crossing the street and hearing a shout behind him at once has the recept, 'hansom cab,' receives it only because he gained it in early childhood. So while 'obvious' (p. 68) is the law of the recept, we have to trace the obvious as made such by repeated efforts in the struggle of existence. Thus by integration mind as hereditary and habitual function is constituted, and thus the bird has a large obvious which is obscure to man, and *vice versa*.

But if the concept originate in activity we need not, as Wundt, make activity the permanent differentiation of the concept. The thoughts which arise spontaneously in thoughtful people are as truly thoughts in their psychological structure as the same thoughts achieved by most powerful effort by the unthoughtful. Thus, 'This is a picture,' is equally a process of thought whether laboriously attained by the child or enounced as matter of course by the parent. So also for the particular idea, a detective who has the recognizing habit recognizes that face with practical spontaneity, which I, who have no aptitude or custom of remembering faces, recognize only with greatest effort. No evolutionary psychologist can emphasize, as does Wundt, activity-passivity as a mark of genera or species. The genetic psychologist holds the real key to classification of psychoses as progressive functions achieved in the struggle of existence and integrated as 'mind,' just as the genetic biologist holds the real key to the classifi-

cation of living forms as the progressive adaptations to environment which become integrated as 'body.' Hence as *versus* Wundt (Lectures on Human and Animal Psychology, p. 146) and Sully (Human Mind, V. 1, p. 389) we cannot allow that mind ever proceeds as mere apprehension, mere intellectualization, but its development as idea is controlled by interest; pain and hunger incite to grasp in presentation and re-presentation and to the abridgment of re-presentation into general idea.

HIRAM M. STANLEY.

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ON AFTER-IMAGES—AN EXPLANATION.

In the last number of the PSYCHOLOGICAL REVIEW Miss Washburn complains that a statement made by me regarding her experiments on after-images is misleading. This statement was that her subjects were drilled to note images similar to her own.

The denial made by Miss Washburn must be accepted as final evidence that such was not the case, although after re-reading her article in *Mind* I do not find that the account there agrees with her later statement. The following quotation from the account of her experiments will serve to indicate that if the statement made is misleading, it is due largely to the lack of clear expression in her article. She says:

"A reliable test could be had of the degree of special practice attained by the subjects in the course of the research and of the influences of the external sources of error. This test lay in the uniformity of the color changes observed in the ordinary unmodified image. A wholly unpracticed observer noticing the course of an after-image for the first time reports chaotic results and no two observers agree as to the alterations in color which occur. *No results were taken account of from any subject until she was sufficiently practiced to find the color changes approximately uniform*[1] or affected only by such fluctuations as could be accounted for from external causes.

"The first point to be determined was the *sequence of colors to be expected* for the ordinary image under these conditions. *This was ascertained by a series of forty experiments made by W.* at the outset of the research; *the other subjects being then practiced* as stated above *till their accounts of the course of the image were consistent.*"

Miss Washburn may have intended to say 'till their accounts of the course of the image became constant' (*i. e.*, of less variability). When her last quoted remarks are coupled with the statement that the *normal* or *expected* course of the image was found from the results of a series of experiments made upon herself, a natural inference is that the subjects

[1] These and other words are italicized by the present writer.

were *expected to see* or to get after-images similar to those of the writer of the article (*which were normal*).

Regarding the fact of individual variability, I regret that my results do not agree with Miss Washburn's. In a subsequent portion of the monograph I report that "One subject saw the after-image always as green; to another it was always red; to a third it varied in the white-black series." The differences did not disappear even after the great number of experiments made by my subjects. In addition, it is worthy of note that these differences occurred when the physical light conditions were very constant—probably more constant than the stimulations given by Miss Washburn, for it is well known that the sky is extremely variable not only in intensity but also in quality (color).

The phenomena are so complex that it would be surprising, to me at least, if there was not great individual variability. The problem may repay a more careful and extended study.

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NEWSPAPER SCIENCE.

The newspapers still occasionally publish an alleged statement of mine which was given such wide currency during the summer to the effect that I proposed within a year or two to 'scientifically demonstrate the immortality of the soul.' It is due to the scientific type of mind as well as the correct point of view from which to consider the phenomena for whose explanation I may fairly demand some theory, to say that no such statement was ever made by me either to a reporter or to any one else. On the contrary I was cautious to the extent of saying that I did not pretend to make any such professions, remembering the fakirs that always have promises of this kind on tap. I do not care to disillusion the newspaper public on this point, as my opinion of it leads me to think that it is past redemption. But scientific people sometimes read the papers and are guilty of believing them to the extent of getting the wrong point of view from which to estimate even important facts. It is the correction of that *point de repere* which I wish to establish. Not that I care for any respect that scientific men are capable of bestowing, as there is a good deal which passes for science that is merely respectable, but illusion nevertheless. But what I do wish understood is that, having found it necessary to make some careful experiments in the Piper case, I found it rational to reverse my preferences between materialism and spiritism, though not profess-

ing to demonstrate anything, even to myself, to say nothing of those who have 'the will to disbelieve.' I have deliberately adopted that position in the face of every imaginable suspicion with the intention of leaving it to the unbeliever of anything supernormal at all to rescue me. The time is past when psychology and science can trifle with the subject of spiritism, no matter what the theories that are adopted to explain its real or alleged phenomena. It must either be accepted or killed. Sneering is no longer effective or scientific Scientific method must control the study of these phenomena or lose its prestige and authority where it has the supreme right, even if it finds it necessary to put illusion and fraud at the very bottom of the universe itself. Personally I do not care what conclusion is reached, especially as in sympathy with Huxley's pessimistic mood I would have a good deal of charity for the desire to boil the human race in Milton's marl of sulphur for awhile and then annihilate it. But this is a matter where wishes avail nothing, and I simply venture to state my facts in due time with the alternative possibilities for their explanation as they present themselves to me, with a preference for that view which keeps science within its chosen field of the finite instead of revelling, like transcendentalism, in an unintelligible infinite. Whether I am right or not I shall leave to others to decide. In the meantime what I shall have to say must not be estimated from the standpoint of fakirs and newspapers. It may or may not have any value, but it does not profess to do any miracles in the way of demonstration.

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PSYCHOLOGICAL LITERATURE.

A Manual of Psychology. By G. F. STOUT. London, W. B. Clive, 1899. The University Tutorial Series. Pp. 643. New York, Hinds & Noble.

Mr. Stout's 'Manual' will at once take rank in the forefront of psychological text-books. It will do so by reason of its admirable readability and clearness of exposition, of its liberal scope of treatment, of its pertinent and practical utilization of daily mental experiences, and of its well-ordered perspective of chapters, as well as by reason of its suggestive originality and its suitability to excite and guide the interest of the student of psychology. These are the qualities that impress the reader whatever may be his special interests or his measure of agreement or disagreement with the general architectural plan of the work or with the particular expression which that plan assumes.

It is fortunately true that the day of independent and antagonistic psychological systems or schools is giving place—has largely done so, in fact—to a recognition of the essential unity of the scope and methods of psychological inquiry; there is approximately, at least, one science of Psychology as there is one science of Physics or Chemistry. The main differences are differences of the attitude or the interests of authors, of the selection of the topics to be emphasized, of adaptation to the particular needs of the public to whom the volume is addressed. A characterization of a new work in these respects will enable the reader to judge of its scope and nature. The common basis of Psychology, its underlying unity, consists in its constant reference to, its dependence upon, its utilization and interpretation of the concrete aggregate of individual reactions of organisms to the environment of which mental life consists. However far away from the starting point our analyses and abstractions may take us, the starting point is at all events well-defined. However it may be with Philosophy, there is no need of bringing Psychology down to earth; to flourish at all it must take root there and expand so far as it has vigor and support, and bear what fruits it may.

The main difference in interest among psychologists is that between a fundamental interest in the mental functions of human and other

organisms, and in the analytical interpretation and explanation of the processes (particularly of the more highly developed and elaborate processes) which these functions inevitably suggest. It is not easy to characterize this difference by an adjective or two. The terms Rational Psychology, Introspective Psychology, Empirical Psychology and others are unfortunate and misleading. To call one 'Physiological Psychology' and the other 'Psychology, Descriptive and Explanatory,' as Professor Ladd has done, is to exaggerate, somewhat, the sharpness of the boundary lines, and perhaps to misplace the lines which mark off the one domain from the other. 'Experimental' and 'Analytic' are another pair of terms that have been called upon to do service for this purpose. Whatever terms may be used they should emphasize the point of view, the directive tendency, the controlling interest, rather than the boundaries of domains of sovereignty of the proposed divisions of Psychology. To the present writer, the term 'Functional Psychology' (in spite of its uncouthness) more aptly than any other, suggests the one interest or sphere of influence; while for the other a term that combines the ideas of analysis and explanation would be appropriate. Each is present in and with the other; foreground and background may shift and be partially interchanged, but the picture is recognizably the same.

Mr. Stout's manual represents the analytic and explanatory interest as uppermost and dominant; but it does so without in any way regarding the other as unnecessary or unimportant. On the contrary the constant dependence of analysis upon observation and experiment is abundantly emphasized by practice if not by precept. Illustrations from animal psychology and from the mental habits of undeveloped races take their place with experiments and observations of introspective human mentality. Nor is such material used merely as illustrative; it serves frequently as the starting point and basis of the analysis which is the main end in view. The selection and perspective of topics reflect this dominant analytic interest. In the discussion of light sensations, theories of color are more fully considered than the phenomena which such a theory might explain; the possible interpretations of the psycho-physic law are more interesting to the author than the facts which underlie the law, or the methods of demonstrating its validity; the same is true of the treatment of spatial perception, of the nervous and motor substrata of conduct and of other important topics. What is thus gained is the concentration of attention on the main problems of psychological analysis; what is lost by the reader is the realization of the steps by which such analysis has

been made probable and may be more completely demonstrated. The sacrifice is, in most cases, justified by the simple fact that it is part of the author's plan and purpose, inasmuch as the scope of his work does not contemplate an adequate consideration of the fact—material and content of psychological lore. One must go to other volumes for such information or for the treatment of the same problems from the 'functional' point of view.

As the work appears in a tutorial series its pedagogical fitness may be properly touched upon. Mr. Stout's position is that sketchiness is above all to be avoided, that the student must 'live himself into psychological problems,' that 'cut and dried statements skimming important questions are of no avail.' These faults the volume avoids and these ends it will accomplish as well as any work that has as yet appeared. For class-room work (which is in a measure opposed to tutorial instruction), systematic statement, and even sketchiness are in part inevitable, in part unobjectionable. But after James no one will hope to satisfy psychological students with anything that is too conventionally cut or shows obvious traces of the drying process. And it is true that the very nature of psychological investigation demands a pliability of attitude, a capacity to observe common occurrences in uncommon ways, which is one of the benefits which a course in psychology should bring about. In this respect Mr. Stout's work is eminently satisfactory.

Mr. Stout further presents an "exposition of Psychology from a genetic point of view. A glance at the table of contents will show that the order followed is that of the successive stages of mental development. The earlier stages have been copiously illustrated by a reference to the mental life of animals. The phases through which the ideal construction of Self and the world has passed are illustrated by reference to the mental condition of the lower races of mankind." This emphasis of the genetic point of view may lead the reader to expect something which the manual does not furnish. There is little in the order of chapters to suggest a specifically genetic treatment as the term is ordinarily understood by students of comparative psychology; what is true is that illustrations of analytic principles and results are drawn from the field of animal minds and primitive human minds with pertinence and effectiveness. The order of chapters, indeed, presents little that is unusual. The introductory chapters present 'the Scope of Psychology,' its 'Data and Methods,' and the general relations of 'Body and Mind.' Book first then considers the general analysis of conscious states, the 'Primary Laws of Mental Processes,' with a sub-

sidiary notice of the theories and weaknesses of 'Faculty Psychology' and 'Associationism.' The next book deals with sensation, taking it up first in its general relation to cognition and experience, to the physiological mechanism upon which it depends, to its interpretation and development and to the 'feeling-tone' which accompanies it; light and sound sensations and the Weber-Fechner law are considered especially in separate chapters while the other senses are briefly, rather too briefly, described. The next division is devoted to Perception and is in many respects the ablest and most valuable part of the work. The treatment falls into two parts: the first or general part considers the nature of perception and the special rôles of imitation, pleasure, pain and the emotions in the development of percepts; the special classes of percepts considered relate to external reality, to the perception of time, and of space as dependent upon touch and sight. The last division takes up concepts and ideas. The treatment is a broad one including chapters on the general relations of ideas to images, on their arrangement in sequences—'trains of ideas,' on their connection with memory and comparison and imagination; on their practical furtherance by language (an interesting chapter) and their significant emotional aspect, on their results in the ideal construction of the world and of self. The last chapter presents a concise treatment of the will in 'voluntary decision.' A chapter which well illustrates the manner in which observations upon animals are utilized in psychological analysis is the general one on perception (Book III., Chapter I.). Chapters VII. and VIII. of Book IV. (on the Self and on Belief and Imagination) as well as Chapter V. (on Language and Conception) furnish the best illustrations of the aid which Analytic Psychology receives from an examination of the mental processes of primitive man. They also illustrate the utilization for the same purposes of the data of mental pathology. But neither this treatment nor these illustrations constitute a genetic point of view as that term is commonly and, it seems, properly understood. A genetic account of sensation would consider the growth and development of sensation from its simplest to its more complex forms; its modifications in successive stages, the sequence of such stages and their dependence upon physiological unfoldment and upon coördinate general psychological processes. Mr. Stout does not consider sensation or other topics in this way; it is not part of the design of the book to treat them in this way. His exposition is quite generally infused with the suggestiveness that comes from a discerning use of genetic data; and that is what must be understood by his 'genetic point of view.'

To indicate Mr. Stout's position on the general problems of Psychology is not a part of the present review. These positions have been indicated by him in his larger and earlier work on 'Analytic Psychology.' The present purpose is to indicate the scope and trend of this excellent tutorial manual to those who consider its use either in the class-room or as collateral reading in connection with a systematic course in psychology. Its use for the former purpose will depend on the nature of the course, the method of instruction and the special interests of the instructor. For the latter purpose its use may be cordially recommended without restriction. As the editor of *Mind*, as the holder of the newly founded readership in mental philosophy at Oxford, as the author of the 'Analytic Psychology' and of this 'Manual,' Mr. Stout's position of leadership and influence in the psychological activity of England is assured; his American colleagues look forward to the fruits of this influence with confidence and with anticipations of aid and inspiration.

JOSEPH JASTROW.

A Theory of Reality. An Essay in Metaphysical System Upon the Basis of Human Cognitive Experience. GEORGE TRUMBULL LADD, Professor of Philosophy in Yale University. New York, Charles Scribner's Sons. 1899.

This volume is a continuation of Professor Ladd's philosophical series, and is the logical outcome of his previous thought as outlined in his *Psychology*, his *Philosophy of Mind* and his *Philosophy of Knowledge*. The *Theory of Reality* is most intimately connected with the *Philosophy of Knowledge*, as is indicated by the sub-title. The author in his introductory chapter contends most emphatically that a systematic metaphysic must be based upon a critical analysis of our cognitive experience, affirming that "the critical theory of knowledge justifies belief in the power of the human mind to know reality and even to give it a measurably consistent, satisfying, and helpful theoretical determination" (p. 27). In his preliminary discussion as to the relation of phenomena to actuality, Professor Ladd holds that the two ideas are not mutually exclusive but complementary and that a phenomenon that is not of and to some real being is inconceivable, while a reality that is not phenomenon to itself or to some other being is unthinkable. Moreover all phenomena must be regarded as standing in some relation to a 'noumenal' self. Every phenomenon then is of some real being—self or thing, and appearing to some cognitive being. This may be regarded as the author's fundamental thesis. In the de-

velopment of this initial position, he proceeds to the analysis of the idea of reality as given in our knowledge of a concrete thing, and he finds that the varied judgments which we form concerning anything in the sphere of reality must be regarded merely as the different expressions of the categories of thought. These categories are not merely thought-categories, but categories of being as well. They are quality, relation, change, time, space and motion, force and causation, quantity and measure, unity and number, form, law and final purpose. There are four characteristics of the categories which are insisted upon as essential to a proper understanding of their nature, and relations. They are, first, "the categories are not separable either in thought, or in reality, as are the concrete realities themselves; second, no single category is recognizable by an analysis of cogntiive experience or is statable in thought without involving the recognition and conception of every other; third, no category is completely resolvable into any other; fourth, all the categories form a sort of interior oneness—a system which appears as a harmony to thought and is experienced as effecting a unity in the world of reality" (p. 85). Moreover, the self is to be regarded as the fundamental reality harmonizing in complete unity all the categories in its being and activity. The reality of things is revealed by their relation to the activity of self. "The experience of being checked, and inhibited on every hand is the very core of my cognition of every other thing. My self-felt activity is opposed by that which is not, and can not be recognized by me, as *my* doing. The inhibition is, on the contrary, necessarily recognized as the doing of that which is not me. To that which, not being my self, stands opposed to my self-felt activity I attribute the same essential being which I know myself to have. *It*, too, is a center of activity which stands to my self-felt activity in the reciprocal relation of acting and being acted upon. It is in this fundamental fact of an activity which is both self-felt and also known to be inhibited that we discover the root, in experience, from which the conception of substance springs forth" (1, p. 123). The reality of things, therefore, is to be conceived after the analogy of the reality of self, and this in many particulars which Professor Ladd dwells upon at some length. For instance, he insists that "to be a real being with actual qualities, is to be what I know myself to be—namely, capable of imitating and of experiencing changes that are attributable to some subject or 'central point of attachment' conceived of after the analogy of a conscious will" (p. 139). And again, "things are known or conceived of as remaining somehow *self*-identical, while being subjects of more or less

important changes, after the analogy of this identity which belongs to the self" (p. 155). These quotations might be multiplied as indicating one of the author's most characteristic positions; indeed, according to his point of view all the categories which function in the consciousness of self operate in an analogous manner in reference to all things which are cognized by self. This analogy is especially emphasized in the categories of form, of law, and of final purpose. And so Professor Ladd insists that "reality in general is known as actually being a unity of force guided by ideas of form and law into processes that conform to ideal ends" (p. 367). He, furthermore, declares that "the forms and laws ascribed by the intellect of man to things have a trans-subjective basis, and are not merely imparted to things by the mental act of knowledge" (p. 347). Finally, the ground of all reality is regarded as the Absolute Being, or the Absolute Self. "We are irresistibly led on from the facts of the interaction of elements under law to the existence of a supreme unity which may serve as a real *locus* for the existence of controlling ideas" (p. 358). All selves and all things have their changing places and functions in the one system because the connection of them all is guaranteed and accomplished by the One Will in its progressive realization of its own Ideas" (p. 362).

These are the salient features, roughly sketched, of Professor Ladd's doctrine. He appears then in the pages of this book as the champion of a trans-subjective reality, conceived after the analogy of that reality which is revealed in the consciousness of self, and which finds its ground and explanation in the all-pervading presence and power of the Absolute Being, the primal reality, and perpetual source of all being whether of a self or of a thing.

In Professor Ladd's general point of view, he has been led to place undue emphasis upon a voluntaristic interpretation of cognitive experience to the neglect of other essential elements which must be reckoned with in laying deep the foundations of a comprehensive and satisfactory epistemological doctrine. In his determination, moreover, of the reality of the not-self, according to the analogy of the self, there is danger of leaving the impression of a crudely anthropomorphic representation of the world and indeed also of the Absolute which underlies the world manifestation.

In finding the essence of reality in the interactions between body and spirit, the author might be forced logically to the position of assuming a like interaction between body and spirit in the being of the One Reality, of which the world, as matter and force, and the vari-

ous selves, as bodies and spirits, are but the partial manifestations. For instance, we read that "the one all-inclusive Being of the World, the Unity of Reality is responsible for the union of body and spirit in each human Self, and of each Self with other selves, and of all selves with all things. If now, however, the language which it has been found necessary to employ in all our explanation of the reality of the self, as dependent for its being and its manifestation upon the Being of the World, is translated over into the thoughts already provided for it, we are led again to the conception of an Absolute Self. For every characteristic of this being of the world, in which all concrete beings 'live and move and have their being,' is constructed after the analogy of the spirit's cognitive and self-active life, in the pursuit of ideal ends" (p. 413). But this 'cognitive and self-active life' of the self, according to Professor Ladd, is a life of joint activity of body and of spirit, and by analogy, the unity of reality would have a being of a like nature.

There is again a danger of losing the self and dissipating its reality by immerging it in the unfathomable deep of the Absolute. Thus, Professor Ladd insists, in the summing up of his argument upon force and causation, that "all the growth of man's cognitive experience reveals the Being of the World as a unity of force, that is constantly distributing itself amongst the different beings of the world so as to bestow on them a temporary quasi-independence, while always keeping them in dependent inter-relations, for the realization of its own immanent ideas" (p. 293). Moreover, Professor Ladd, in attempting a solution of this problem of the relations sustained by the human will to the will of the Absolute, says that "there is not necessarily any more contradiction involved in this so-called 'double aspect' of the relations of man to God than is involved in the consideration of all particular existences from both the scientific and the ultimate, or metaphysical, point of view. Two of H unite with one of O to form the compound H_2O '*because of*' the laws of chemical affinity and 'because of' the relations into which the H and O are brought by the compelling forces of their environment, temperature, pressure, induced molecular activities, etc. But the chemical is not the entire explanation of such a transaction in reality. Really H_2 and O come together in this way, because 'it is their nature'; the ultimate explanation takes into account the mysterious being of these elements as a primary postulate, a pre-condition of all the forms and laws of their reciprocal behaviors. Now from philosophy's point of view this is essentially no other than the position H and O behave

in this way because it is the Will of the Absolute that they should so behave" (p. 515). The impression left upon the reader is that the seemingly independent will of personality is in reality none other than the 'quasi-independence' which may be attributed to things. Surely this is a loss of the self as regards the integrity of its free personality. The relation of the Absolute Will to the wills of persons must certainly differ from the relation of the Absolute Will to the manifold forces of Nature.

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Einleitung in die Vergleichende Gehirnphysiologie und Vergleichende Psychologie mit Besonderer Berüchsichtigung der Wirbellosen Thiere. By JACQUES LOEB. Leipzig, J. A. Barth, 1899. Pp. 207 and 39 illustrations.

This work from the hands of Professor Jacques Loeb of the Department of Physiology of the University of Chicago is of considerable significance for psychologists. It embodies not only the results of many original researches, but is also a valuable digest of recent literature bearing on the subject. Its importance is enhanced by its negative criticism of current neurological conceptions of cerebral localization and its psychological corollaries. As the author says in the preface, this work grew out of an examination of the assumption made in his previous work[1] that all the reflexes of the animal life are under the specific direction of ganglion cells. This assumption was rendered assailable by the fact discovered by him of the identity in nature of many of the animal and plant tropisms. He was led to one of two conclusions. Either we must attribute consciousness to plant forms, or we must develop a new criterion for the presence of consciousness in lower animals. As we shall see, he adopts the latter alternative.

He takes up first the consideration of the fundamental facts and general principles of comparative brain physiology. The physiological unit has hitherto been regarded as the reflex, while ganglion cells have been regarded as the structural element which makes the reflex possible. Even writers who have seen nothing in the reflex but a mechanically determined activity have regarded ganglion cells as the essential organs of the complicated movements involved therein. Says Professor Loeb, "I never would have doubted the correctness of the older physiology, which gives to the ganglion cell this important rôle, had not my own discovery of the essential identity of the animal and

[1] Especially his 'Der Heliotropismus der Thiere und seine Ubereinstimmung mit dem Heliotropismus der Pflanzen,' Würzburg, 1890.

plant heliotropisms shown the inadequacy of this point of view, and at the same time given me another conception of the real nature of the reflex." This new point of view led him to further experimentation in which these animal heliotropisms are shown to remain even when the central ganglia supposed to be concerned have been removed. In other words, there is a direct relation between the peripheral sense organs and the different musculatures, not mediated by the central nervous system. The general conclusion from his experiments, therefore, is this, that stimulability and conductivity, only, are necessary for reflexes, and that these are universal properities of all protoplasm. Ganglion cells possess no mysterious property, not common to all protoplasmic nervous tissue, for the production of the reflex. The ordinary conception of instincts as inherited reflexes perpetuated by a sort of organic memory in these ganglion cells comes in for its share of criticism. Such a theory can furnish neither a mechanic of instincts nor a natural explanation of their transmission. Their real explanation is found in the phenomena of the various tropisms (heliotropism, chemotropism, geotropism, stereotropism, galvanotropism, etc.). If, then, the mechanics of a number of instincts can be traced back to tropisms common to plants and animals, and if the significance of ganglion cells is limited to the function of conductivity, then we must expect a new statement of the factors which condition these complicated reflexes. Researches on the galvanotropism of animals leads Professor Loeb to conclude that a simple relation exists between the orientation of the nervous elements in the central nervous system (along the line of the chief axis of the body) and the direction of the movement of the body called out through these elements. Tropism reveals itself in two general conditions (1) in the specific irritability or excitability of definite peripheral elements, and (2) in the symmetry of the body. Symmetrical elements of the organism have the same irritability; unsymmetrical elements have different irritabilities. The elements near the oral pole have a greater stimulability than the aboral, or the reverse. The same argument applies to the so-called spontaneous movements, as well as to reflexes. There are two groups of these, (1) simple spontaneous movements, and (2) rhythmical spontaneous movements. That simple spontaneous movements do not require mediation by ganglion cells is apparent, *e. g.*, from the spontaneity of the swarm-spores of algæ. The rhythmical spontaneous movements are more important in this connection. Respiration and heart-beat belong in this category. These are ordinarily attributed to certain ganglia. But the researches of Gaskell, Engel-

mann, Loeb, and others, show that these processes can take place independently of the ganglia to which they have been referred. They do not require a higher coördinating center, but are purely segmental reflexes. This segmental conception is then extended to the whole animal series.

Naturally there arises the question of the psychological application of the foregoing conclusions. It is one of the distinctive problems of brain physiology, in the opinion of Professor Loeb, to develop a theory of consciousness. Accordingly he makes a statement of what he regards as the criterion for the determination of the presence of consciousness in the lower animals. This he finds in what he calls 'associative memory.' By this he means that arrangement through which not only the immediate reactions take place which correspond to the stimulus and the sense organ, but also other effects or reactions which are the normal responses of other stimuli acting either previously or simultaneously on the organism. This had been worked out previously by Professor Loeb in his 'Beiträge zur Gehirnphysiologie der Würmer.'[1] The detailed experimental application of this criterion to all the different classes of animals constitutes the future problem and task of comparative psychology.

On the side of the comparative physiology of the brain the chief object of this work is, thus, to establish the segmental theory of the central nervous system. On the side of comparative psychology, its principal object is to establish 'associate memory' as the criterion for the presence of consciousness in the lower animals. With this general view of his spirit and aim we may pass to a brief survey of some of the facts upon which these conclusions are based.

The first type that we have presented to us is *Medusa*. In those forms of Medusa which possess a more highly developed nervous system in the marginal ring (such as Hydromedusa) the rhythmic spontaneity is localized exclusively in that part of the animal containing the nerve ring, though in types possessing a less highly developed nervous system (such as Aurelia aurita) any portion of the umbrella is capable of exhibiting these contractions. It is fundamental that the continuity of the connections of the nerve ring is requisite for the production of the synchronous movements which are necessary for the contraction of the umbrella. An analogous example is found in the case of the frog's heart, in which it has been found by experimenting on the contractility of different excised portions, that its different parts contract in rhythmic unison with the sinus venosus, which

[1] Pflüger's Archiv. Band 56, 1894.

when isolated, has the greatest rate of contraction. There is no necessity here for a special coördinating center. The stimulation of one part of the organ results in the instantaneous communication of the stimulus to the remaining parts, and the organ practically contracts as a unit. This is confirmed by Dr. Hargitt's researches (under Professor Loeb's direction) on Gonionemus in which two individuals were artifically united, and two days after the operation showed perfectly synchronous contractions. In opposition to Romanes, Professor Loeb denies that these contractions are dependent upon the presence of ganglia, either in the case of *Medusa* or in the case of the frog's heart, and this denial is supported by Engelmann's conclusions that the seat of the automatic activities even of the adult heart is the muscle tissue rather than the ganglia. Professor Loeb cites further the case of the Infusoria in which we have rhythmic contractions of the vacuole without the presence of distinct nerve elements. Such reactions as that of the frog in attempting to remove the acid irritant have been attributed to the complex coördinations made possibly by its relatively highly differentiated nervous system. But Professor Loeb shows that entirely analogous reactions may be secured in the case of Hydromedusa in which the nerve ring has been excised.

The next researches are on the central nervous system of *Ascidians*. The peculiar 'purposive' reflexes of Ciona intestinalis, ordinarily explained as due to the coördinating functions of its single ganglion, are here shown to remain after its excision. The ganglion has simply the function of conductivity, rendering such reactions quicker, but is not essential to the possibility of such reaction.[1] Similarly, in such reflexes in the higher animals as the contraction of the iris, the function of the central nervous apparatus is demonstrated to be simply the accelerating of the reaction, since contractions take place not only in the decerebrated animal, but in the excised iris. That other tissues beside nervous tissue are capable of such contractions is, again, substantiated by the acknowledged phenomena of contractility under similar conditions in insectivorous plants and in protozoans, where there is no suggestion of a clearly differentiated nervous tissue.

In the *Actinarians* (Actina equina) the following experiment was performed. A bit of thread with a piece of meat attached to one end and a bit of paper to the other was laid across its tentacles. In every case, no matter what the relative position of the two ends of the thread, the meat was drawn inwards through the mouth opening while

[1] This is reproduced from his previous work entitled 'Untersuchung zur physiologischen Morphologie der Thiere,' II., Würzburg, 1892.

the paper was allowed to remain inertly hanging outside. Here the explanation is similar to that of the movements of carnivorous plants. There is no need for the assumption of the coördinating function of a ganglionic center. The chemical affinity between the constituent elements of the food material and the tentacular structure is sufficient explanation. That there is no purposive consciousness or intelligence here is proved by further investigations of heteromorphosis, in which, for example in Cerianthus membranaceus, a new ring of tentacles became artificially developed in connection with a lateral incision. Here the tentacles attempted, as in the case of the true mouth, to force the morsel of the meat into the digestive cavity at the point where the mouth should have been. This occurred as often as the meat was presented to it. There was no suggestion of associative memory, no learning by experience. It was altogether as mechanical as any purely physical or chemical reaction. Indeed, in the case where the new head was developed near the old one, the two sets of tentacles actually entered into a contest each striving to force the morsel of food into its own mouth cavity. In addition to these chemotropic reflexes, the foot of the actinian is shown to be stereotropic, and the general bodily position determined geotropically. An actinian placed in an inverted position in a test-tube, with its head end down, will regain its normal upright position within an hour by the gradual contracting of its body and tentacles. That this is due to the sheer force of gravitation is shown by Professor Loeb's experiments with a horizontally placed coarse screen through which the actinian which was laid upon it slipped slowly into a suspended vertical position in a purely passive way. When the screen was inverted, instead of slipping out, the foot end began to bend and finally slipped through another mesh of the screen, and so on, until the animal lay coiled among the meshes of the screen.

The reactions of *Echinoderms* are shown to belong to the same general category. The ventral surface of the starfish is stereotropic, while in some types there is combined with this a negative geotropism or positive heliotropism which causes these animals to climb up the slant surfaces of rocks or up the vertical walls of the aquarium. It is necessary for the nerve ring to be intact in order that the animal may regain its normal position after having been laid upon its back; but this is not because of any disturbance of a ganglionic center since a single amputated arm will succeed in regaining its original position as well as the normal starfish.

In his researches on *Planarians* Professor Loeb emphasizes in

the first place the absence of any reactions such as in the higher animals are called reactions to pain. If the head end of Thysanozoon Brocchii be snipped off as it floats upon the surface of the water, the tail end drops inertly to the bottom, while the head end (containing the only ganglion of its simple nervous system) proceeds on its way unhindered and without any signs of pain from the operation. The peculiar feature about the orientation of these animals is that on the ventral side we have positive stereotropism and on the dorsal side negative stereotropism. Here again the reactions take place quite as well in the absence of the ganglion as when it is present, except that they are slower. If a cross-section be made which is not quite complete, the head end which contains the ganglion will attempt to creep off (spontaneous movements) while the tail end fastens itself to the nearest object (positive stereotropism). In this type the head end only shows the spontaneous movements. But in a fresh water Planarian, Planaria torva, though morphologically very closely related, the tail end as well as the head end creeps off in a very lively fashion in spite of the absence of the ganglion. The spontaneity of progressive movement is evidently here again not a function of the 'brain' or cerebral ganglion. The reactions of Planaria torva to light are also of especial interest. They take place as well in the decapitated as in the normal animal.

The same question arises in the experiments on *Annelids*. Has the cerebral ganglion any function of coördination in connection with the movements involved in locomotion ? This is shown to be not the case. Even in the dog it is shown by Goltz that coördinations for locomotion are explicable in a manner similar to that in worms, *i. e.*, without calling upon the inexplicable functioning of some ganglion. As in *Annelids* the progressive movements of locomotion take place not only in the absence of the cerebral ganglion, but even when the chain of segmental ganglia is itself broken, so in the dog certain locomotor reflexes occur in the hind legs even after cross-section of the spinal cord. In both cases these reactions are due to reflected stimulation through the peripheral organs (skin, muscles, etc.). The difference between the dog and the earthworm, so far as locomotion is concerned, is not so much the difference in the central nervous apparatus as in the development of the peripheral organs. Had the dog, instead of its long lithe limbs, mere stumps instead, we would have the same phenomena and the same explanation as in the progressive movements of the earthworm. Other functions of the earthworm, as in the case of Planarians are explained as due to heliotropism or chemotropism.

The researches on the foregoing forms have taught us, says Professor Loeb, that the characteristic reactions of these animals are determined (1) by the different forms of stimulability and arrangement of the peripheral elements, and (2) by the ordering of the motor organs, the muscles. The central nervous system has no other part to play than that of furnishing a means of quicker and easier conduction, and the fact of the greater complexity of the cerebral ganglion is simply due to the massing of the peripheral sense organs in the head. He then goes on to show how the segmental conception holds also for *Arthropods*. His first example is Limulus polyphemus, in which all of the nervous system was extirpated with the exception of a portion of the circumœsophageal ring and the abdominal (respiratory) ganglia. The portions of the nervous system retained enabled the animal to eat and to breathe, and the animal was actually kept alive for some time by food being furnished it. This completely overthrows the conclusions of previous investigators who asserted the existence of a special control by the cerebral ganglion. The relations in higher animals are essentually the same as those in Limulus. The usual reference of the coördinating center for the respiratory movements to the medulla, to the point called by Flourens the 'vital node' is altogether erroneous. This view has rested upon the observation that an injury at this point or section of the cord between this point and the point of origin of the Phrenicus leads to the arresting of the respiratory movements. But Professor Loeb shows that this is a temporary inhibition only like that occurring in Limulus immediately after the removal of the subœsophageal ganglion. Langendorff,[1] indeed, has made the important discovery that even the decapitated vertebrate, in which the 'vital node' is removed,is capable of independent respiratory movements. In fact, if in the new-born vertebrate the respiration be kept up artificially until the shock effects pass off, the animal will then continue to breathe in a normal manner. To sum up, the common idea, as represented by Faiore's work, that the centers for the reflexes of locomotion, breathing, etc., are located in the subœsophageal or prothoracic ganglia, in arthropods, is wholly rejected, and the conclusion of Bethe adopted, that these reflexes actually have, as they appear to have, their centers, in the corresponding segmental or abdominal ganglia. Or, to state the principle in general terms, each individual segment of a segmental animal may be viewed as a simple reflex animal like the Ascidian. The assumption of special higher coördinating centers is wholly superfluous.

[1] Studien über die Innervation der Athmenbewegungen, I., Mittheilung, Archiv für Physiologie, 1880.

The researches on *Mollusks* are wholly in line with what proceeds. The probability is, then, that the segmental theory holds also for *Vertebrates*. It is Professor Loeb's opinion that the brain represents more, rather than less, segmental ganglia than even investigators sympathetic with this point of view have been inclined to assign to it. Experimentation shows that after recovery from the shock effects of operation, the caudal part of the spinal cord is capable of all the reflexes that are possible when it possesses its normal connection with the cephalic portion (brain). Such reflexes as reaction to irritation of the skin, erection of the penis, secretion of urine, dejection from the rectum and bladder, and the vasomoter reflexes are all retained. Loeb has shown the same to be true in the case of the Crawfish for the respiratory movements, and Goltz has shown the same for the arm reflexes in the frog, while Schrader has shown the same for the movements of locomotion. The cerebral cortex is found to be unnecessary for vision in frogs and fishes: the optic thalamus only is necessary. The theory of a single ruling center or even of a group of cortical controling centers is thus negatived, and the importance of the independent functioning of the spinal segmental ganglia is emphasized. The chief reasons why the segmental theory has not been accepted hitherto, according to Professor Loeb, are (1) because in vertebrates the brain shows the segmental character only in its earlier embryological stages; consequently only a few brain physiologists have seen that this is the key to the understanding of its function, (2) because the shock effects of operation on the higher animals has so obscured the real effects of extirpation, and (3) because of the influence of a metaphysical psychology with its doctrine of separate functions and corresponding centers.

Professor Loeb's theory of animal instincts grows out of this segmental conception of the nervous system. He looks upon instincts as made up of a series or chain of segmental reflexes (Kettenreflexe). By instincts are generally understood unconscious actions of animals directed toward a definite end. Such are the habits of insects which lay their eggs in material which serves later as food for the hatching young. Such also are the periodic migrations of birds and aquatic animals, instincts of self-protection, protection of the young, etc. But many of these so-called 'purposive' instincts are clearly shown by Professor Loeb to be explicable as animal tropisms. His whole chapter on Instincts is very interesting and instructive as throwing light on the segmental theory. Of course, his theory of instincts militates against any theory which suggests that instincts are degraded habits,

the inheritance of fixed modes of action of preceding generations of ancestors. In connection with the question of instincts arises the problem of the transmission of characters That there is a transmission through the sexual or reproductive cells is not doubted, but the question that has puzzled all investigators is how so simple a structure as a reproductive cell can be the bearer of so complicated an apparatus as that necessary for reflexes and instincts. There are two possibilities. Either the egg is a much more complicated structure than has been supposed, containing all the elements necessary for the fully developed animal, or the complexity of the mechanism of reflexes and instincts can be reduced to the terms of simple elements and processes which can be easily transmitted by the egg without the latter possessing such a highly complicated structure. Weismann represents the former standpoint. The results of Professor Loeb's investigations lead him to adopt the latter alternative. Growth processes are first of all processes of chemical differentiation, with which the particular morphological characters are secondarily associated. The egg is the bearer primarily simply of certain determinate chemical substances, and hence the transmission of characters is possible only in the form of the transmission of these substances, and the nervous system can influence this transmission only as it may be instrumental in changing their nature. Chemical theories of heredity are not new, but in the form in which Professor Loeb presents his theory it has a place quite its own.

This brings us to Professor Loeb's conception of the criterion of consciousness. He finds this, as has been said, in 'associative memory.' Following Mach he identifies what we call self-consciousness or the I-consciousness with the memory factor of our experience, and finds the cerebral cortex to be the medium of the associations involved in the more complex reactions of the higher animals. It is no more remarkable that in certain animals we should have a special apparatus for associative memory than that we should have in certain animals a special apparatus for bringing rays of light to a focus for visual perception. In applying this criterion it is found that consciousness is present in most mammals and some lower forms. The dog responds to its name and greets its master joyously, the parrot learns to speak, the dove to return to its cote or to the place of feeding. Even the tree-frog is granted to have consciousness, though the common frog is denied it. To some fishes it is granted, though in the shark its existence seems doubtful. For the presence of associative memory in invertebrates Professor Loeb thinks there is very little evidence. It

may be granted to spiders, certain crabs, and cephalopods; but is denied absolutely to cœlenterates and worms. Especially does Professor Loeb oppose what he calls the anthromorphism of such writers as Romanes, Eimer, Nagel, etc. He finds almost all previous work on ants and bees an overdrawn account of their supposed intelligence, and cites the work of Bethe in support of his own. Many experiments are cited in support of the final conclusion that 'associative memory,' and hence consciousness, is to be regarded as confined to a relatively small part of the animal kingdom. It is not possible to go into the details of these experiments within the limits of this review, but the *facts* seem to be essentially as represented by Professor Loeb. The only query is, whether Professor Loeb's interpretation on the psychological side is the only *explanation of these facts*. Shall we deny a lower form of consciousnses to these simpler types of animal life simply because we do not find the cerebral apparatus present which conditions our highly differentiated mode of consciousness? May it not be granted that in these lower types associative memory plays a very small part in the life of the animal, and that when it does come it comes in flashes only, in connection with certain exigencies of life, without denying to them either the possibility or the actual presence, at times, of the beginnings of this function? Such researches as those of Professor Loeb no more prove the absence of lower modes of consciousness than the facts of habit in human experience militate against a doctrine of the freedom of the will. A man is no less free because his volitions are regular and rationally systematic. The same is true of the lower animals. Because we have found a statement for the activities of an eel's tail in terms of physics and chemistry, this does not prove that those activities are not also interpretable in another way, provided we can have the chain of psychological inferences unbroken, as in our attribution of consciousness to a fellow human. Are there any greater difficulties connected with the interpretation of consciousness as between man and the dog, than, for example, between the dog and the eel? What we call the voluntary or relatively free acts of the higher type of consciousness, whether it be dog or man, are in no way different psychologically as acts than the tropisms of the lower animal and plant forms, only we have a chemico-physical statement for the latter, whereas we have not as yet, because of the relative incompleteness of our science, an analogous statement for these highly complicated reactions of the higher type of animal. We should be inclined to meet Professor Loeb's encroachments into the realm of the higher animals by not alone granting him what he claims of animal activities (all the invertebrates and

most of the lower vertebrates) for such mechanical or chemico-physical explanation, but we should grant him the whole field, all the activities of the higher animals including man—only, we should insist that after he had covered the whole ground, his explanation of the facts would still be partial only, requiring to be supplemented by what we may call the psychological and teleological interpretation of these same facts. It does not follow, in other words, that there is no conscious side to these phenomena of the activity of the lower animals and plants simply because these activities have been discovered to be mechanically uniform and necessary. On page 162 he cites the facts of the various kinds of tropism in plants and animals as belonging to the same general category as the chemical reaction, say, of the photographic plate, and suggests that since we attribute no consciousness to the latter, there is just as little reason to attribute consciousness to the former. The reply is this: There is not only just as little reason, but there is also just as great reason for attributing consciousness, not alone to the photographic plate, but to all nature as well. This is just the contention of a current idealism in philosophy, that the whole of nature is susceptible of a psychological and teleological interpretation, as well as of a physical or purely mechanical explanation. This whole argument which is employed with such telling effect to prove the utter absence of consciousness may be turned against itself. The error lies, not in the accuracy with which the experimental observations have been made, nor, in general, in the valuable array of facts set forth, but in the *point of view* from which these facts are interpreted, and in the implicit assumption that the physical and mechanical explanation (the 'scientific' explanation, in the narrow sense) is the whole of the matter. There certainly is no reason for assuming the presence of that type of consciousness which is marked by 'associative memory,' in the absence of the cerebral mechanism which is its invariable condition in the higher animals. On the other hand, it is quite as unjustifiable to deny altogether the possibility of any psychological interpretation for these lower forms of activity. Proceeding upon the lines of Professor Loeb's argument, when we have found a physico-chemical statement of the conditions of all human action, 'associative memory' will go out of the same door that has already, in his mind, closed behind consciousness in the lower animals. The steadily increasing indebtedness of biology to physics and chemistry does, indeed, as Professor Loeb contends, cut at the root of unfounded psychological myths such as the theory of a spinal soul, crude pan-psychism, and hasty generalizations

on hylozoism. But this relation of dependency on the physical sciences is to be supplemented by an equally great and increasing dependency, not alone of biology, but also of chemistry and physics, on the psychological point of view. If psychology seems to be losing its position as a distinct science, it is not because the psychological mode of viewing phenomena is decadent; rather, it is because this psychological mode of interpretation is becoming applied to all spheres of activity, supplementing, and, when rightly understood, reinforcing the mechanical or physical interpretation in every department of scientific procedure. Psychology is losing the narrow footing which it once held among the sciences, not to disappear as the superstition or myth of an unscientific age, but to become universalized as the indispensable correlate of all the so-called natural or physical sciences. But after all this has been said, Professor Loeb has certainly rendered a real service, not alone to physiology, but also to psychology, in overhauling in so thorough a manner the unintelligent lumping of all psychical functions in the brain, which has marked so much of the previous work in this field.

H. HEATH BAWDEN.

VISION.

On After-images. SHEPHERD IVORY FRANZ. Monograph Supplement to the PSYCHOLOGICAL REVIEW, No. XII. New York, Macmillan. June, 1898. Pp. iv+61. Price, 75 cents.

The present volume is, in the main, an attempt to deduce the factors influencing the perception of after-images. This empirical portion comprises sections dealing with the threshold, the latent-period, the duration, the fluctuations, the space-relations and the retinal transfer.

Experiments upon four subjects show that to have an after-image appear seventy-five per cent. of the times the eye is stimulated it is necessary to have as a stimulus (*a*) for $\frac{1}{100}$ second and 64 sq. mm. surface of light, an intensity of $\frac{2}{25}$ candle power; or (*b*) for one second exposure. an area of 4 sq. mm. and an intensity of $\frac{2}{25}$ candle power; or (*c*) for one second exposure, an area of 64 sq. mm. and a light of $\frac{1}{100}$ candle power.

The latent period (*i. e.*, the time between the end of the stimulus and the first appearance of the after-image) was found to vary correspondingly with changes in the objective stimulus. The lesser times, the greater areas and lesser intensities seem to make this period short,

whilst opposite conditions of stimulation increase the time. This difference is explained as due to the large image for a large area, to the blinding effect of an intense light and to the movement of the eyes if the stimulus is prolonged. Under the different conditions of the test, the latent period was found to vary from .46 second to 3.4 seconds.

The total and the actual duration of the after-image were found to be dependent in like manner upon the area, the intensity and the duration of stimulation. Changing the intensity from .01 to 1 candle power showed that the after-image remained longer under the second condition. The increase was from 32 to 68 seconds and from 25 to 55 seconds respectively for total and actual durations. Increasing the area from ¼ to 4 sq. cm. gave the following results: 28 to 58 seconds for total time; 22 to 44 seconds for actual duration. A change in the duration of the stimulus from 1 second to 10 seconds caused an increase in duration of the after-image from 40 to 68 seconds total duration, 29 to 53 seconds actual duration. The effect of the larger areas is considered to be almost entirely due to the attention, while the greater effect of time and intensity increases may be considered largely physiological. When the retina was stimulated by lights of different colors, but of uniform intensity, the duration of the after-image was not changed. The peripheral portions of the retina seem to have after-images of less duration than what was found for the fovea and adjacent parts. This, it is suggested, may be due to the attention rather than to any special difference in the nervous mechanism. It was found that after-images lasted longer with those individuals who had good eyesight and with those who were considered to be visualizers. 'Bright' boys had after-images of longer duration than did their fellows. Fatigue, imagination, voluntary attention were found to affect the duration of the phenomena. A consideration of the various results indicates that the greater durations are due largely to the attitude of the observer—to his attention—and the physical light-changes have a comparatively minor effect.

Contrary to what was found by other investigators the fluctuations were not found to be constant with any one physical condition. The variation is great and seems to be dependent upon factors other than the changes in the stimulus.

The color-changes and the changes in intensity of the after-image were not constant with any change in the stimulating light, and the great individual differences make the phenomena very puzzling.

The spatial character of after-images is considered briefly. Numerous experiments with naïve subjects show that if suitable stimuli be

presented *and the mind prepared* for the detection of the effect of solidity, the tridimensional character is evident. This would class the appearances partly as perceptual.

The various views regarding the so-called 'retinal transfer' are noted: (1) that the phenomenon is only a form of binocular contrast; (2) that there is a transfer from the stimulated to the unstimulated eye; (3) that the image has a central origin; and (4) that the transfer is only apparent. The numerous facts adduced in support of each view are considered, the results of new experiments are given, and the conclusion drawn that the image has its seat in the stimulated retina, and that the 'transfer' is apparent.

This empirical portion is followed by a descriptive account of after-images, relating them particularly to memory-images, to imagination-images, and to sensations and perceptions. In a concluding section there is given a brief historical résumé of the various theories proposed to explain the phenomena from the time of Aristotle to the present day.

A short bibliography of thirty-one titles is appended.

THE AUTHOR.

Gefühlston und Sättigung der Farben. JONAS COHN. Philos. Stud., XV., 279–286.

In 1894, Cohn maintained, in an article published in the Philos. Stud., X., 562–603, that the pleasantness of colors increases with their saturation, that the more saturated colors are the pleasanter. A few months later, in an article reporting an investigation made in Professor Titchener's laboratory it was said, "we cannot confirm Cohn's rule that the more saturated colors are the pleasanter; individuals differ in this respect." In the recent article, Cohn publishes the results of a set of experiments carried on to account for the different results obtained in the two cases. In the later experiments Cohn used color materials similar to those used in the Cornell laboratory, viz.: the Bradley colored papers. His method was to repeat our experiments according to the serial method as we used it, and to supplement this by the method of comparison by pairs.

Cohn's article may be summed up as follows: The method of absolute individual judgments, as used by Major, is manageable, observers easily become accustomed to the method. Yet, the conditions of judging by the method of comparison by pairs are simpler and, in general, this method is preferable. The regular arrangement of the colors is objectionable since it gives rise to a demand for change. The

order of shades or tints within the same color should vary. If one wishes to determine the influence of saturation one must place together the shades which are nearly alike in brightness.

Results.—The saturated colors are preferred in a majority of cases, yet some persons prefer the less saturated colors. Two suggestions are made in explanation of the rarer class of judgments; it is due either to a varying retention of the original sense feelings, or to the influence of association. Of the two explanations Cohn inclines toward the second. More light could be thrown on the problem by experimenting with a great number of the exceptional type under varying conditions, and by bringing in children.

It seems to the present writer that while Cohn's recent investigations tend to confirm his earlier statement that saturated colors are pleasanter we must still hold that the rule is not universal: there are great individual differences. And, indeed, Cohn admits this in the present article.

D. R. MAJOR.

HEARING.

Über die maximale Geschwindigkeit von Tonfolgen. Von OTTO ABRAHAM und KARL SCHÄFER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, XX., 6, 408–416.

Über das Abklingen von Tonempfindungen. Von OTTO ABRAHAM. Pp. 417–424.

The authors produced the tones for their investigation by means of a siren—a disc with openings. They got the result that a trill of only two pitches requires a duration of about 35σ of either tone, to be recognized as a trill. When the duration is shorter, not a trill is heard, but a duad of tones. The shortest duration of the tones forming a trill does not depend either upon the absolute pitch or the interval. When instead of a trill a musical figure of four or five notes is used, a duration three times as long as above of each tone is required, in order that the right succession of the pitches may be recognized.

In the second paper Abraham states that the comparatively long duration (35σ) of the tone is conditioned by the auditory *after-sensation*. Alfred Mayer tried to determine the duration of the after-sensation of a sound by the method of interrupting the tone of a tuning fork by periodically closing and opening a pipe, through which the tone is heard. No doubt the duration of the after-sensation cannot be determined by this method. But the incorrectness of the method

cannot be proved by Abraham's argument. This is based on the assumption that higher tones of an equal intensity of stimulus have a greater intensity of sensation than lower ones. I have claimed in the *Zeitschrift* (Vol. XX., p. 18) that this assumption is by no means self-evident, but must be proved. The mere appeal to authorities such as Helmholtz and König does not prove anything. To prove his assumption, Abraham had to give an exact definition of 'intensity of stimulus.' This he defines as (the variation of) 'the pressure of the air, *i. e.*, the amplitude of the particles of air,' as if 'amplitude' and 'pressure' were identical. Of course, the variation of pressure being represented by a curve, the *amplitude of the curve* would be equivalent to the variation of pressure. But the *amplitude of the particles of air* is something quite different.

Abraham further alleges that the amplitude of the membrane of the Edison phonograph is proportional to the amplitude of the tone. But what is meant here by 'amplitude of the tone'!—amplitude of the particles of air or variation of pressure? Probably the latter, because the amplitude of the membrane, without regard of the frequency of vibration, is proportional to the variation of the pressure, but not to the amplitude of the particles of air. Now Abraham concludes: If the velocity of the cylinder is changed, only the pitch of the tone is changed, but the variation of the pressure of air remains unchanged, because the amplitude of the membrane is unchanged. That this conclusion is wrong, any physicist who knows the elements of mechanics could have told him. Although the amplitude of the membrane, when forced into vibrations by the shifting pressure of the air, without regard of the frequency is proportional to the variation of pressure; the variation of pressure, when caused by vibration of the membrane, is *not* independent of the frequency of vibration. The former is the case because the natural tone of the membrane of the phonograph is considerably higher than the usual musical tones; the latter, because the velocity of the propagation of air waves is greater than the maximum velocity of the vibrating membrane. Consequently the higher tones produced by the phonograph were stronger than the lower ones, because the stimuli were stronger. The same is true of the experiments of Helmholtz and König, quoted by Abraham.

The real argument against Mayer's method is quite a different one, which Abraham has totally overlooked. When—as in Mayer's experiments—a tone of 64 vibrations is interrupted 25 times, each short stimulus consists only of 1.3 vibrations, or if we regard the fact that the tone, when theoretically kept from the ear, practically acts upon

it with a small intensity, each stimulus consists of 2.6 vibrations. But since 2 vibrations at least are required in order to produce a sensation of tone, how can 2.6 vibrations (although varying somewhat in intensity) cause the sensation of tone to rise and fall in intensity. For this effect naturally at least 3 vibrations are required. So it is a matter of course that the tones in Mayer's experiments at last became continuous; but no duration of an after-sensation was determined.

Abraham is in so far right, as he states, that, to determine the duration of the after-sensation of a sound, two tones of different pitches must be used, and not only one periodically interrupted. What Abraham and Schäfer have determined, is, in any case, not the whole duration of the sensation, but by far the greater part of it. So we notice the interesting fact that the shortest duration of a certain sensation (over 35σ) is about 60 times as long as the duration of the shortest stimulus (0.6σ), by which the sensation is produced.

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Die Bestimmung der unteren Hörgrenze. Von KARL L. SCHÄFER Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. XXI., 3–4, 161–173.

The lowest tones, as other tones, may be produced by vibrations of the air of a corresponding frequency, or they may be difference tones or tones of intermittence. Schäfer used the two last methods in order to determine the lowest tones audible. These methods have the advantage that overtones cannot make the result questionable, because difference tones and tones of intermittence are simple. The lowest difference tone he heard with certainty corresponded to 27 vibrations. In one case he thought he heard a difference tone corresponding to 14 vibrations.

I may add that, when I made the same experiments (not published) some years ago, the lowest difference tone I heard corresponded to 17 vibrations. [The lowest tones of intermittence that Schäfer heard while using the third of the methods above mentioned corresponded to 16 vibrations.] The possibility of hearing such low tones depends greatly on the intensity of the stimulus.

MAX MEYER.

TASTE.

Geschmack und Chemismus. Von WILHELM STERNBERG. Zeitschrift für Psychologie und Physiologie der Sinnesorgane 20, 385–407, 1899. Also Archiv für Anatomie und Physiologie (Physiologische Abteilung), 1899. 367–371.

The author holds that there are only two kinds of taste sensations —sweet and bitter—and that all other forms of taste are 'heterogeneous sensations of touch or combinations of such with taste.' Disregarding for the present the intensity of the sensations produced, he seeks to answer the following questions from a consideration of the composition of various chemical compounds and the taste they produce: (1) Why do certain substances possess taste and why are others that are not less soluble tasteless? (2) Why do some substances taste sweet and others bitter? (3) The psychophysical question: Why is sweet agreeable and bitter disagreeable?

Briefly stated, the general conclusion reached is that taste-producing substances have a 'double nature,' and that when the particular saporific groups contained in the molecule are arranged 'harmoniously' with regard to their number sweet taste results, when this 'harmony' is lacking bitter taste is produced.

In the case of organic substances the saporific groups are considered to be OH and NH_2. The former must be in combination with an alcoholic radicle and the latter with a carboxyl group; but "in order that sweet taste may result, the alcoholic radicle and the OH group must be harmoniously linked with respect to their number." Again, the amido and the carboxyl groups must be in *a* or *o* position with respect to each other to preserve this 'harmony' and so produce a sweet taste. As special examples for illustration are cited the following facts: polyatomic alcohols are sweet. If in them methyl or more hydroxyl groups are introduced they still remain sweet, but the introduction ot phenyl results in the formation of a bitter compound. The introduction of carbonyl or aldehyde groups in the polyatomic alcohols, forming sugars, increases the sweet taste. But if in the sugars phenyl is introduced bitter substances result; thus the glucosides which are largely phenyl derivatives of glucose are bitter. Resorcine and hydroqinone are sweet, and pyrocatechin and pyrogallic acid are bitter. The amido acids are sweet as are also dulcine, the condensation product of urea and phenetol, and especially saccharine. Quinine is the bitterest substance, but by introducing ethyl carbonic ester into its molecule a tasteless compound results.

The inorganic compounds of those elements that are in the middle of the periodic or natural system and that have neither strongly pronounced positive nor negative characteristics are sweet, and compounds of the elements of the other groups are bitter.

The paper contains no data of special experiments. The author bases his theory—if it may be so termed—upon a collection of facts largely well known. Many of the additional allusions, etymological explanations, etc., that the article contains may be interesting, but they are hardly to the point and do not serve to strengthen the author's theory.

As far as the 'double nature' is concerned upon which, according to the author, saporific power depends, it may with propriety be held that all chemical compounds (except the molecules of the elements themselves) have a dual nature, since, speaking in general terms, it is *unlike* elements that combine to form chemical compounds. Again the author explains only in a vague, indefinite way what he means by the 'harmony' in the compound upon the existence of which sweet taste depends. By his arbitrary assumption that there are only two taste sensations, he eludes the insurmountable difficulties of explaining the taste of salty, sour and alkaline substances by means of his theory.

Though the article under consideration is interesting to read, the reviewer must confess that in his humble judgment it has not answered in any satisfactory manner the three important questions that it has raised.

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TOUCH.

Ueber die Function der Tastkörperschen. M. von Frey and F. Kiesow. Zeitsch. f. Psych. u. Phys., XX., p. 126.

This is a very careful investigation of the stimulus to touch sensation. The following facts "necessitate the assumption that, as in the field of the other senses, so also for the sense of touch, the external stimulus acts only as a liberator of energy (auslösend); that the energy in the nerve-fibers peculiar to the excitation-process arises, not from the activity of the stimulant, but from chemical transformations in the end-organ, of which the stimulant is merely the occasion."

1. The depression-energy necessary to excite the peripheral nerves is several hundred times greater than that of the weakest touch-

stimulus. 2. Continuous pressure stimulates the touch-organs but not the peripheral nerves. 3. After a heavy and not too brief weight upon the skin, the sensation continues after the outer stimulus has been removed.

The authors investigate this process of liberating energy (auslösungsvorgang), the factors involved being: (1) the locus of the stimulation, (2) its quantitative aspect, (3) the depth of the depression and (4) its time-aspect. They eliminate the last factor, wherever the surface stimulated remains constant, by keeping the time of the depression-process constant. Where the surface stimulated varied, the time was varied according to a rule resulting from experiments previously carried out by Dr. Kiesow. The rule is as follows: with constant pressure, increase in the surface area stimulated necessitated a slow proportional increase in the rapidity of the depression: decrease of the surface-area necessitated a rapid proportional increase in rapidity of depression.

As to the locus of stimulation. Areas differ: (1) in the sensitiveness of their end-organs, and (2) in the number of their end-organs. Places free from hair must be sought out, as using the razor does not remove the difficulties which the hair-cells involve. The problem demanded that the experiments be carried out by stimulating single touch-organs, and this is possible only on such portions of skin as do not contain too many touch-organs. Tendons and blood-vessels, concave and convex surfaces had to be avoided. The place chosen was the volar side of the wrist. After carefully searching out the loci of the touch-organs, the area experimented upon was drawn upon a chart, each point being carefully marked and described.

After the time and loci of stimulation had thus been arranged for, the author's simplified problem related to the significance: (1) of the surface area, and (2) of the depth of the impression in the stimulation of single anatomical elements. As, however, the depth of the depression was empirically adjusted to the varying anatomical characteristics of the skin (the authors find an empirical adjustment the only possible one), the main question investigated was the significance of the quantity of stimulated surface to touch-sensation.

The apparatus for applying stimuli was but slightly different from the hydrostatic threshold-scales described by Dr. Kiesow in a previous report. Metal pieces were carefully cut to correspond to the size of single elements or touch-organs beneath the surface of the skin, and to various combinations of single elements. For single end-organs the optimum demanded was 0.4 mm.^{2}or a circle of 0.5 mm. diameter.

Stimulations covering 0.05 mm.²—0.005 mm.² were affected by the use of hairs. The disturbing effects of weariness, varying conceptions of the nature of the experiments in the mind of the subject, variations of temperature in the room, etc., were carefully eliminated. The two authors experimented upon each other as subjects.

The main results of the investigation are as follows: for the stimulation of any particular touch-organ the production of a certain depression with constant time and depth characteristics at the point of skin beneath which the organ is located is the necessary condition. The excitation of a touch-organ is a function of the depression necessary at its locus. The barely noticeable depression increases slowly but clearly when the surface area is increased; and this generalization holds for large as well as for small stimulated surfaces. Where the time is constant, such stimuli as produce a like depression possess equivalent stimulation-values.

One wishes that these careful and elaborate experiments had been carried out on a larger number of subjects, and that central influences such as attention, preconception and suggestion had been investigated. The question arises whether the Druckgefälle of a particular touch-organ, here treated as a constant, does not vary with central influences which the authors do not take into account.

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FEELING.

Zur Kritik der Wundt'schen Gefühlslehre. E. B. TITCHENER. Zeitschrift für Psych. u. Phys. d. Sinnesorgane, Bd. XIX. Pp. 321–326. 1899.

Bemerkungen zur Theorie der Gefühle. W. WUNDT. Philosophische Studien, Bd. XV., Hft. 2. Pp. 149–182. 1899.

The more often an expert opinion is expressed, the more liable is it to suffer variation and possible inconsistency. This danger is more pronounced when the expressions are offered in independence of one another. Variations are sometimes due, no doubt, to a progress in the views of the expert, as well as to the modifying influence of newly discovered facts. Inconsistencies may arise through forgetfulness or simple logical weakness. The literature under consideration is of interest because such tendencies are more or less apparent in it. Wundt has given three definite expressions of his psychological views[1]

[1] *Grundzüge*, etc., 4th ed., 1893, I.: 555–600; *Lectures on H. and A. Psych.* (Eng. tr. based on 2d German ed.), 1896, pp. 210–222, 247f.; *Outlines of Psych.* (Eng. tr.), 1897, pp. v–vi, 74–89. Cf., also Titchener, *An Outline of Psych.*, 1896, Ch. V., especially sec. 34, pp. 105–108.

(nine, if we count the several editions of the works and omit reference to the articles appearing in the *Studien* and elsewhere), which, in so far as they relate to the nature of feeling, seem to Titchener to be at variance, both among themselves and with the observable facts in the given set of phenomena.

The sole aim of the writer in I. is to bring forward a few objections against Wundt's theory of the attributes of simple feelings. These criticisms must be derived from introspection and general reasoning, chiefly because, as it is alleged, Wundt has defended his views on no other grounds, and, in part, because of the lack of experimental data in this field. Wundt distinguishes three chief 'directions' of the primary qualities of feeling, designated by the following pairs of terms, which indicate their opposite extremes: 'pleasurable' and 'unpleasurable,' 'arousing' and 'subduing,' 'strain' and 'relaxation.' (*Outlines*, pp. 82, 83.) Each one of these principal directions is supposed to contain an unlimited number of variable but nameless qualities.

The critic brings forward the following objections against the theory: 1. The logical arrangement of the effective contrasts is in error by implying negation. Pain is not the absence of pleasure. The classification is an attempt to bring together two entirely different things, viz., adherence to the logical scheme of feeling-opposite feeling, and faithfulness to reports of introspective observation. The second and third contrasts are, however, less apparent in immediate experience than the first contrast. 2. In the *Lectures*, pleasure-pain is presented as the preëminent qualitative characteristic of the entire content of consciousness, while the two remaining contrasts are connected with the intensity and the time-rate, respectively, of sensations and ideas. This is merely an arbitrary multiplication of the chief directions of the feelings. The spirit of Wundt's classification is culpable for leaving out the *spatial* aspects. If we have a world of temporal relations, one of intensities, and one of qualities in our life of feeling, then we must also allow a world of spatial relations. The critic suggests the direction of 'expansion-contraction.' This soon betrays itself as an invented process, which speaks against it, and also against the whole classification within which it would have a place logically. In the *Outlines*, the directions are as stated above; the first contrast referring to the momentary state of consciousness, the second to the succeeding states, the third to the determinations which carry over from preceding states. The reconciliation of these two expositions is a stumbling block to the critic. That contained in the

Outlines is in agreement with Wundt's general theory, but is the less probable, and a daring hypothesis which is unsupported, except, perchance, by facts not yet discovered. 3. Wundt's theory is negatived by an alleged discovery of those pertinent facts, gained by the introspection of a student of psychology at Cornell during 1897–8. This student, knowing Wundt's theory well, brought to light the result that at no time during the year of observation was there an emotional content, excepting pleasure-pain, which he could not locate exactly in some of his bodily organs, *i. e.*, every feeling was discriminated as a sensation-complex. Thus the patient thinking of the expert is brought to zero with the greatest dispatch, in a presumed hope of working out a statement of a problem that may be treated only by physiological and experimental methods!

In II., Wundt replies, with his usual leisure, in terms of fact and in terms of logic. 1. The objections of the critic are based upon an erroneous idea of the reasons which led to the formulation of the three-dimensional scheme. 2. The exposition of the several views is not free from errors and misunderstanding. 3. The test which the critic applied to the theory in the way of observation, gives rise to considerable doubt.

These points are specifically worked out in such a manner as to show that the character of the treatment in the *Lectures* and *Outlines* ought to have held back the criticism that the scheme had no other ground than mere reasoning. The critic omits, in a remarkable way, all recognition of the experimental observations used in the first exposition (*Grundzüge*, I., 563ff.). The first part of the reply gives a selective review of the results of Lehmann's plethysmographical study of the bodily expressions, presenting six curves which are almost entirely corroborative of the theoretical analysis in the *Outlines*, which was based, in part, on the studies of Mentz, Mosso, Kiesow and others. Wundt insists that "any distinction of various dimensions of feeling is 'up in the air' so long as its foundation is not placed directly in the subjective observation of the feelings," the real basis of his own scheme (p. 165). Wundt again admits that there are certain *Lücken* in his analysis of the composite forms of emotion, which can be filled up by further experimental investigation. He also finds suggestive corroboration of his fundamental analysis of feeling in Vogt's inquiries into hypnotic states of consciousness. With reference to the logical reductions by the critic, the reply runs, that etymology cannot serve the problems of psychology, nor can there be any further argument where one party does not find, by introspection, any positive

state known as 'relaxation,' etc. Spatial relations did not appear in the original schemes simply because (not alone of their unintelligibility in this connection) they do not appear in either immediate observation or the expressive movements. The apagogical method of the critic involves a complete denial of any quality, or direction whatsoever, as belonging to simple feeling. The reply also finds the accidental introspection of the student to instance no improvement upon the old method of psychologists, for a failure to note which the critic cannot be excused. The sweeping conclusions drawn from the data gained by this method are untenable, being supported by the mere logical supposition of an opposition of sensation and feeling.

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EDUCATIONAL PSYCHOLOGY.

The Physical Nature of the Child, and How to Study it. By STUART H. ROWE. New York, The Macmillan Co. 1899. Pp. xiv+207. Price, $1.00.

This book is an excellent manual of the simpler tests of the senses, motor ability and bodily conditions, written by one actively engaged in school work and intended in the first instance for teachers. Of its scope the author says in his introduction: "The design of this book is not to give the latest results in each department of child-study touching its physical side, but to give teachers practical hints which may call attention to some physical peculiarities of children, not commonly investigated in their bearing on the work of the schools." The tests are such as can be easily made without special apparatus and in the case of possible ailments of a serious nature are intended only to select pupils to be submitted to expert examination at the hands of a physician.

The first few chapters deal with sense tests; these are followed by those on motor ability, enunciation, nervousness, fatigue, diseases, habits of posture and of movement, growth and adolescence, and school and home conditions affecting the child's physical nature.

The work closes with a bibliography of about a hundred titles and a full index. A few minor errors may be noted, as, for example, on p. 13, where the statement about the width of the small letter *n* is obviously intended for the width of the lines of the letter, on p. 27, where the acoumeter is described as an instrument arranged to tap on *wood*, and on pp. 20 and 21 where the description of the tests for color-blindness, if not absolutely misleading, could be decidedly improved. The

book as a whole, however, is practically unique in its field and admirably suited to its purposes.

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Psychologie mit Anwendung auf Erziehung und Schulpraxis. DR. KARL HEILMAN. Leipzig, Verlag der Dürr'schen Buchhandlung. Pp. 86.

This third edition of this small work appears after seven reprints of the second edition. It treats of the life of cognition, the life of feeling and the life of volition, embracing 32 sections in all. Illustrations and drawings are used. The elements and essentials of each topic are followed in each instance by a paragraph of application to education and instruction. The psychology is not only elementary, in many particulars it is rather old; but the essentials of each discussion are outlined in simple and compact form. The pedagogical applications made by the author are unexpectedly praiseworthy for so brief an exposition. As in the psychological section, so also in the pedagogical section, one finds only the alphabet of the subject, but these rudiments are very forcibly and helpfully presented. The book is not a pseudo-scientific popularization; it is, on the other hand, filled with fundamental truths and suggestive observations gathered from both experience and study.

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ETHICS.

A System of Ethics. By FRIEDRICH PAULSEN. Edited and Translated from the Fourth Revised and Enlarged Edition. By FRANK THILLY. New York, Charles Scribner's Sons, 1899. Pp. XVIII., 723.

The English reader will feel himself under a burden of obligation to Dr. Thilly for his painstaking and very readable translation of Professor Paulsen's masterly treatise on ethics. Although not written for the delectation of philosophers, who as the Professor somewhat cynically remarks, 'are already overburdened with ideas,' the book is one which the philosopher may read with profit; while to the intelligent lay public that is interested in practical questions, it will furnish a rich and varied treat. To readers of this class the somewhat loose and unconventional style will be a recommendation, while the good sense, catholic spirit and full acquaintance of the author with his

theme, will carry conviction where a more labored and technical argument would fail. The translation covers the first three parts of Professor Paulsen's work, leaving untranslated Book IV. on 'The Forms of Social Life,' the first three books treating of the history of ethical conceptions, the fundamental concepts and principles of ethics, and the doctrines of virtues and duties.

The historical review is very thorough and evinces the author's extensive acquaintance with the development of ethical thought. His opinion is that the Greeks have long ago discovered the fundamentals of moral science and his aim in the history is to show how the Greek conceptions, temporarily displaced by the introduction of Christianity, gradually reasserted themselves, though in a more or less profoundly modified form, as the shaping ethical norms of modern thought, while, therefore, Professor Paulsen does not assign the supreme place to Christianity in the ethical movement which some would claim for it, he treats it on the whole with sympathetic insight. Modern ethical thought beginning with Hobbes and marked in its beginning by a strong reaction against theological ethics has developed mainly along two characteristic lines, what Professor Paulsen calls formalistic intuitionism on the one hand and teleological hedonism or Utilitarianism on the other, Kant being the great exponent of the former, while the English utilitarians from Locke to Mill represent the latter.

In Book II. Professor Paulsen defines his own theory as *Teleological Energism*, thus putting himself in line with the best Greek tradition and especially with the theory of Aristotle. Ethics is teleological in the sense that its central category is that of the good, while it is energistic rather than hedonistic, inasmuch as the direct end is not happiness or pleasure, but perfection of the life activity. On this basis Professor Paulsen develops his own doctrine of the leading ethical exceptions and in connection with it an important criticism of opposing views. It would be impossible to follow him through the discussions of such topics as the highest good, pessimism, duty and conscience, egoism and altruism, the relation of morality and religion, the freedom of the will and self-control. Professor Paulsen is largely in sympathy with the doctrine of evolution and his tendency is to seek in the stream of social and political development for the growing content of morality. The moral is the national or race consciousness asserting itself in the conscious life of individuals and its content develops as a body of customs (sitten) which take the form of rules, that impose themselves as objectively binding. On the subject of the relation of religion to morality, Professor Paulsen mediates between two opposing

views. While refusing to allow that morality rests ultimately on religion, he claims for religion a very important historical function in the development of morality and he is also of the opinion that religion is a very important agent in keeping morality alive and vigorous.

Book III. treats of the leading virtues and duties under such topics as the bodily and economic life, spiritual life and culture, honor and love of honor, compassion and benevolence, justice, love of neighbor and veracity. It is in this sphere of applications that Professor Paulsen's peculiar endowment of discriminating good sense shows itself most clearly. One cannot follow his perfectly candid and sincere discussions without getting a great deal of help in the solution of his own practical problems. In fact, the principal value of the book consists in the practical wisdom which illumines almost every page, and it will be likely to be most helpful to the one who comes to it not for an authoritative enunciation of principles but for helpful suggestions in the treatment of practical moral issues.

One may be disposed to admit the general tenability of Professor Paulsen's doctrine without agreeing with him on special points. Doubtless, both the hedonist and the intuitionist could defend themselves against his criticisms, and the special student could, no doubt, urge good reasons in favor of a more rigid and technical treatment. For ourselves we prefer to take Professor Paulsen as he is and professes to be, not as a philosopher's man who is aiming at a strict formulation of principles, but as one who prefers to take the intelligent lay reader into his confidence and discuss with him in an unconventional way the practical questions of his every day life.

A. T. Ormond.

PHILOSOPHY, PSYCHOLOGY AND PHYSICS.

Address to the Mathematical and Physical Section of the British Association for the Advancement of Science at the Dover Meeting, September, 1899. J. H. Poynting, F.R.S. *Science*, N. S., Vol. X., pp. 385–396; *Nature*, Vol. 60, pp. 470–474.

There is evident in many places a movement tending to unite philosophy with science after a century of partial separation. Psychology, which has become a science without definitely breaking off from philosophy, has played a somewhat important part in this movement, and the psychologist may at least claim a maieutic function in a rebirth of the utmost importance for the advancement of knowledge·

Professor Poynting's address at the recent meeting of the British Association is a good example of the *rapprochement* referred to, and is especially interesting because he feels justified in claiming that he represents a point of view in which physicists are tending to agree.

For Professor Poynting 'causes' should be abolished in physical science, being appropriate only on the psychical side; 'laws' do not prescribe the course of nature, but are descriptions more or less exact; heat may be more real than kinetic energy, hypotheses, such as those of atoms and of the ether, are not on an equal footing with phenomena observed by the senses, but are temporary scaffolding, ejective rather than objective. Professor Poynting's address is so clear and concise and at the same time so accessible that an abstract is unnecessary, and I shall confine this notice to two points open to question.

Professor Poynting says: "The special molecular and etherial machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight." Now we undoubtedly do, most of us, live chiefly in a visual world, but I believe that our world of physical science has been chiefly derived from the kinæsthetic senses. In the visual world the same space varies in size, parallel lines converge, uniform motion may be any kind of motion, etc. It is probably the senses connected with touch and movement that have led to the assumption that matter and motion are real, while color, etc., are secondary qualities, and also to the anthropomorphic ideas of laws and causes.

In the second place Professor Poynting's claim that mechanical description applies to non-living, but not to living matter appears to me invalid. If the movements of an animal are teleological, none the less so is the distribution of light, heat, moisture, soil, air, etc., which makes these movements possible. Certainly we can prepare a Nautical Almanac years in advance, while we cannot surely predict the next movements of an amœba. But the time element is not essential; the solar system may simply change more slowly. We cannot predict what solar system will arise from a new nebula. Perhaps we can describe and predict the motions of the solar system, because we need to concern ourselves with only one solar system which happens to be quite a simple and permanent affair; whereas there are innumerable living things each mechanically far more complex than the solar system.

J. McK. C.

NEUROLOGY.

Sur le siege des images motrices. Dr. Paul Hartenberg. Revue de Psychologie, April, 1899. Pp. 109–115.

This article (by one of the editors of the new magazine whose alleged purpose it is to afford physicians the benefits of modern psychology) discusses briefly the 'motor image' from the modern or physiological view-point.

An image in the sense here employed is, "before all, a special nervous mechanism, or the harmonious activity of a group of nervous elements, or finally, a functional synthesis of neurons"; a motor image is then, in particular, the neural mechanism which presides at the performance of a movement, and especially the cortical part of this process. This motor image is to be clearly discriminated both from an image of a movement and from the 'cortical representation of a movement.' The term motor image, Dr. Hartenberg thinks, should be applied strictly to the *cortical* motor mechanism or process. In articulating a word, for example, there is activity in three neural centers: in the bulbar centers of execution (motor roots of the pneumogastric, hypoglossal, spinal accessory and facial nerves); cortical centers for projection (the common motor zones for movement of the lips, tongue, larynx, respiratory apparatus, etc.); and a cortical center of association (Broca's convolution, foot of the left third frontal). To the last of these alone the term motor image should be applied, thinks the writer, for it is here that the essential process of executing a movement takes place, here alone the elementary impulses being combinable; here it is that they are coördinated and really made into a movement.

The place of this process in general seems to be on the confines of the area of the association center of Flechsig, on the marginal zones; as yet only the centers of articulation and of writing are exactly located.

In conclusion, the author considers any value which the neuron theory may have to assist understanding of cerebral activity. Psychical activity, he thinks, is infinitely more complex than the simple physical extension and retraction of parts of a cell: "these phenomena, admitting that they are true, aid one's understanding perhaps, but they explain nothing, for they must themselves be explained. * * * We have as yet not discovered, in the microscope, the secret of human thought."

George V. N. Dearborn.

Ein neues Algesimeter. Professor Dr. W. v. Bechterew. Neurologische Centralblatt, 1 May, 1899, No. 9. Pp. 386–390.

This new algometer devised by Professor Bechterew, of St. Petersburg, will have certain value in the laboratory of normal psychology, and will prove of considerable importance, probably, in researches on abnormal subjects, especially in 'nervous clinics.' The first part of the article describing it is devoted to a discussion of the merits and defects of like instruments already in use.

The new algometer consists essentially of a brass cylinder and a flat disk at the end about 1.5 cm. in diameter. The center of this plate is pierced, and though it extends on occasion a sharp steel point, released by pressure on a trigger and extended by force of a spring. The degree of its extension beyond the disc is regulated by turning the latter, the degrees being marked around the cylinder. Thus the instrument is useful in all degrees of paralgesia up to analgesia. Several forms of point and surface can be screwed to the handle in place of the one described, and besides the plain handle, a dynamometer with a circular dial reading up to 500 grms. is furnished to which as a handle the end pieces already mentioned may be screwed; this dynamometer adds very largely to the general usefulness and desirability of the instrument. Richter, of St. Petersburg, makes the set and sells it at 25 roubles.

George V. N. Dearborn.

NEW BOOKS.

Nouvelles reserches sur l'esthétique et la morale. J. P. Durand. Paris, Alcan, 1900. Pp. 275. 5 fr.

Science sociale et democratie. G. L. Duprat. Paris, V. Giard and E. Brière, 1900. Pp. 320.

La doctrine de Spinoza exposée et commentée a la lumière des faits scientifiques. Emile Ferrière. Paris, Alcan, 1899. Pp. x + 357. 3 fr. 50.

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NOTES.

THE American Psychological Association is holding, at Yale University, its annual meeting as we go to press. In spite of the fact that a number of the leading members are abroad the meeting promises to be successful, about forty papers having been already offered. The discussion will be on 'The Teaching of Psychology,' and will be opened by Professors Fullerton, Jastrow, Aikens and Judd. The address of the president, Professor John Dewey, and the Proceedings will be published in the next issue of the REVIEW.

THE subject for the discussion before the American Society of Naturalists at its New Haven meeting during Christmas week is 'The position that universities should take in regard to investigation.' The American Psychological Association is represented by Professor Jastrow.

THE Provisional International Committee on a catalogue of scientific literature met in London last August and decided to exclude psychology, except in so far as it can be included under physiology, and also decided to abandon the proposed card catalogue. It seems strange that the Provisional Committee, containing no psychologist or representative from America, should have excluded psychology from the list of the sciences after it had been incorporated by two International Conferences. In any case the abandonment of the card index makes the catalogue of comparatively little interest to psychologists, we having already two excellent annual bibliographies.

MISS LILLIE J. MARTIN has been appointed assistant professor of psychology at Stanford University during the absence abroad of Professor Angell.

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THE PSYCHOLOGICAL REVIEW.

PSYCHOLOGY AND SOCIAL PRACTICE.[1]

BY PROFESSOR JOHN DEWEY,
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In coming before you I had hoped to deal with the problem of the relation of psychology to the social sciences—and through them to social practice, to life itself. Naturally, in anticipation, I had conceived a systematic exposition of fundamental principles covering the whole ground, and giving every factor its due rating and position. That discussion is not ready to-day. I am loath, however, completely to withdraw from the subject, especially as there happens to be a certain phase of it with which I have been more or less practically occupied within the last few years. I have in mind the relation of Psychology to Education. Since education is primarily a social affair, and since educational science is first of all a social science, we have here a section of the whole field. In some respects there may be an advantage in approaching the more comprehensive question through the medium of one of its special cases. The absence of elaborated and coherent view may be made up for by a background of experience, which shall check the projective power of reflective abstraction, and secure a translation of large words and ideas into specific images. This special territory, moreover, may be such as to afford both sign-posts and broad avenues to the larger sphere—the place of psychology among the social sciences. Because I anticipate such an outcome, and because I shall make a survey of the broad field from the special

[1] Address of the President before the American Psychological Association, New Haven, 1899.

standpoint taken, I make no apology for presenting this discussion to an Association of Psychologists rather than to a gathering of educators.

In dealing with this particular question, it is impossible not to have in mind the brilliant and effective discourses recently published by my predecessor in this chair. I shall accordingly make free to refer to points, and at times to words, in his treatment of the matter. Yet, as perhaps I hardly need say, it is a problem of the most fundamental importance for both psychology and social theory that I wish to discuss, not any particular book or article. Indeed with much of what Dr. Münsterberg says about the uselessness and the danger for the teacher of miscellaneous scraps of child study, of unorganized information regarding the nervous system, and of crude and uninterpreted results of laboratory experiment, I am in full agreement. It is doubtless necessary to protest against a hasty and violent bolting of psychological facts and principles which, of necessity, destroys their scientific form. It is necessary to point out the need of a preliminary working over of psychological material adapting it to the needs of education. But these are minor points. The main point is whether the standpoint of psychological science, as a study of mechanism, is indifferent and opposed to the demands of education with its free interplay of personalities in their vital attitudes and aims.

I.

The school practice of to-day has a definite psychological basis. Teachers are already possessed by specific psychological assumptions which control their theory and their practice. The greatest obstacle to the introduction of certain educational reforms is precisely the permeating persistence of the underlying psychological creed. Traced back to its psychological ultimates, there are two controlling bases of existing methods of instruction. One is the assumption of a fundamental distinction between child psychology and the adult psychology where, in reality, identity, reigns; viz.: in the region of the motives and conditions which make for mental power. The other is the assumption of likeness where marked difference is the feature

most significant for educational purposes; I mean the specialization of aims and habits in the adult, compared with the absence of specialization in the child, and the connection of undifferentiated status with the full and free growth of the child.

The adult is primarily a person with a certain calling and position in life. These devolve upon him certain specific responsibilities which he has to meet, and call into play certain formed habits. The child is primarily one whose calling is growth. He is concerned with arriving at specific ends and purposes—instead of having a general framework already developed. He is engaged in forming habits rather than in definitely utilizing those already formed. Consequently he is absorbed in getting that all around contact with persons and things, that range of acquaintance with the physical and ideal factors of life, which shall afford the background and material for the specialized aims and pursuits of later life. He is, or should be, busy in the formation of a flexible variety of habits whose sole immediate criterion is their relation to full growth, rather than in acquiring certain skills whose value is measured by their reference to specialized technical accomplishments. This is the radical psychological and biological distinction, I take it, between the child and the adult. It is because of this distinction that children are neither physiologically nor mentally describable as 'little men and women.'

The full recognition of this distinction means of course the selection and arrangement of all school materials and methods for the facilitation of full normal growth, trusting to the result in growth to provide the instrumentalities of later specialized adaptation. If education means the period of prolonged infancy, it means nothing less than this. But look at our school system and ask whether the 3 R's are taught, either as to subject matter or as to method, with reference to growth, to its present demands and opportunities; or as technical acquisitions which are to be needed in the specialized life of the adult. Ask the same questions about geography, grammar and history. The gap between psychological theory and the existing school practice becomes painfully apparent. We readily realize the extent to which the present school system is dominated by carrying over

into child life a standpoint and method which are significant in the psychology of the adult.

The narrow scope of the traditional elementary curriculum, the premature and excessive use of logical analytic methods, the assumption of ready-made faculties of observation, memory, attention, etc., which can be brought into play if only the child chooses to do so, the ideal of formal discipline—all these find a large measure of their explanation in neglect of just this psychological distinction between the child and the adult. The hold of these affairs upon the school is so fixed that it is impossible to shake it in any fundamental way, excepting by a thorough appreciation of the actual psychology of the case. This appreciation cannot be confined to the educational leaders and theorists. No individual instructor can be sincere and whole-hearted, to say nothing of intelligent, in carrying into effect the needed reforms, save as he genuinely understands the scientific basis and necessity of the change.

But in another direction there is the assumption of a fundamental difference: Namely, as to the conditions which secure intellectual and moral progress and power.[1] No one seriously questions that, with an adult, power and control are obtained through realization of personal ends and problems, through personal selection of means and materials which are relevant, and through personal adaptation and application of what is thus selected, together with whatever of experimentation and of testing is involved in this effort. Practically every one of these three conditions of increase in power for the adult is denied for the child. For him problems and aims are determined by another mind. For him the material that is relevant and irrelevant is selected in advance by another mind. And, upon the whole, there is such an attempt to teach him a ready-made method for applying his material to the solution of his problems, or the reaching of his ends, that the factor of experimentation is reduced to the minimum. With the adult we unquestioningly assume that an attitude of personal inquiry, based upon the possession of a problem which interests and absorbs, is a necessary

[1] I owe this point specifically (as well as others more generally) to my friend and colleague, Mrs. Ella Flagg Young.

precondition of mental growth. With the child we assume that the precondition is rather the willing disposition which makes him ready to submit to any problem and material presented from without. Alertness is our ideal in one case; docility in the other. With one, we assume that power of attention develops in dealing with problems which make a personal appeal, and through personal responsibility for determining what is relevant. With the other we provide next to no opportunities for the evolution of problems out of immediate experience, and allow next to no free mental play for selecting, assorting and adapting the experiences and ideas that make for their solution. How profound a revolution in the position and service of text-book and teacher, and in methods of instruction depending therefrom, would be effected by a sincere recognition of the psychological identity of child and adult in these respects can with difficulty be realized.

Here again it is not enough that the educational commanders should be aware of the correct educational psychology. The rank and file, just because they are persons dealing with persons, must have a sufficient grounding in the psychology of the matter to realize the necessity and the significance of what they are doing. Any reform instituted without such conviction on the part of those who have to carry it into effect, would never be undertaken in good faith, nor in the spirit which its ideal inevitably demands; consequently it could lead only to disaster.

At this point, however, the issue defines itself somewhat more narrowly. It may be true, it is true, we are told, that some should take hold of psychological methods and conclusions, and organize them with reference to the assistance which they may give to the cause of education. But this is not the work of the teacher. It belongs to the general educational theorist: the middleman between the psychologist and the educational practitioner. He should put the matter into such shape that the teacher may take the net results in the form of advice and rules for action; but the teacher who comes in contact with the living personalities must not assume the psychological attitude. If he does he reduces persons to objects, and thereby distorts, or

rather destroys, the ethical relationship which is the vital nerve of instruction (*Psychology and Life*, p. 122, and pp. 136–138).

That there is some legitimate division of labor between the general educational theorist and the actual instructor, there is of course no doubt. As a rule, it will not be the one actively employed in instruction who will be most conscious of the psychological basis and equivalents of the educational work, nor most occupied in finding the pedagogical rendering of psychological facts and principles. Of necessity, the stress of interest will be elsewhere. But we have already found reason for questioning the possibility of making the somewhat different direction of interest into a rigid dualism of a legislative class on one side and an obedient subject class on the other. Can the teacher ever receive 'obligatory prescriptions'? Can he receive from another a statement of the means by which he is to reach his ends, and not become hopelessly servile in his attitude? Would not such a result be even worse than the existing mixture of empiricism and inspiration?—just because it would forever fossilize the empirical element and dispel the inspiration which now quickens routine. Can a passive, receptive attitude on the part of the instructor (suggesting the soldier awaiting orders from a commanding general) be avoided, unless the teacher, as a student of psychology, himself sees the reasons and import of the suggestions and rules that are proffered him?

I quote a passage that seems of significance: "Do we not lay a special linking science everywhere else between the theory and practical work? We have engineering between physics and the practical workingmen in the mills; we have a scientific medicine between the natural science and the physician" (p. 138). The sentences suggest, in an almost startling way, that the real essence of the problem is found in an *organic* connection between the two extreme terms—between the theorist and the practical worker—through the medium of the linking science. The decisive matter is the extent to which the ideas of the theorist actually project themselves, through the kind offices of the middleman, into the consciousness of the practitioner. It is the participation by the practical man in the theory, through the agency of the linking science, that determines at

once the effectiveness of the work done, and the moral freedom and personal development of the one engaged in it. It is because the physician no longer follows rules, which, however rational in themselves, are yet arbitrary to him (because grounded in principles that he does not understand), that his work is becoming liberal, attaining the dignity of a profession, instead of remaining a mixture of empiricism and quackery. It is because, alas, engineering makes only a formal and not a real connection between physics and the practical workingmen in the mills that our industrial problem is an ethical problem of the most serious kind. The question of the amount of wages the laborer receives, of the purchasing value of this wage, of the hours and conditions of labor, are, after all, secondary. The problem primarily roots in the fact that the mediating science does not connect with his consciousness, but merely with his outward actions. He does not appreciate the significance and bearing of what he does; and he does not perform his work because of sharing in a larger scientific and social consciousness. If he did, he would be free. All other proper accompaniments of wage, and hours, healthful and inspiring conditions would be added unto him, because he would have entered into the ethical kingdom. Shall we seek analogy with the teacher's calling in the workingmen in the mill, or in the scientific physician?

It is quite likely that I shall be reminded that I am overlooking an essential difference. The physician, it will be said, is dealing with a body which either is in itself a pure object, a causal interplay of anatomical elements, or is something which lends itself naturally and without essential loss to treatment from this point of view; while the case is quite different in the material with which the teacher deals. Here is personality, which is destroyed when regarded as an object. But the gap is not so pronounced nor so serious as this objection implies. The physician after all is not dealing with a lifeless body; with a simple anatomical structure, or interplay of mechanical elements. Life functions, active operations, are the reality which confronts him. We do not have to go back many centuries in the history of medicine to find a time when the physician attempted to deal with these functions directly and immediately. They were so

overpoweringly present, they forced themselves upon him so obviously and so constantly that he had no resource save a mixture of magic and empiricism: magic so far as he followed methods derived from uncritical analogy, or from purely general speculation on the universe and life; empiricism so long as he just followed procedures which had been found helpful before in cases which somewhat resembled the present. We have only to trace the intervening history of medicine to appreciate that it is precisely the ability to state function in terms of structure, to reduce life in its active operations to terms of a causal mechanism, which has taken the medical calling out of this dependence upon a vibration between superstition and routine. Progress has come by taking what is really an activity as if it were only an object. It is the capacity to effect this transformation of life activity which measures both the scientific character of the physician's procedure and his practical control, the certainty and efficacy of what he, as a living man, does in relation to some other living man.

It is an old story, however, that we must not content ourselves with analogies. We must find some specific reason in the principles of the teacher's own activities for believing that psychology—the ability to transform a living personality into an objective mechanism for the time being—is not merely an incidental help, but an organic necessity. Upon the whole, the best efforts of teachers at present are partly paralyzed, partly distorted, and partly rendered futile precisely from the fact that they are in such immediate contact with sheer, unanalyzed personality. The relation is such a purely ethical and personal one that the teacher cannot get enough outside the situation to handle it intelligently and effectively. He is in precisely the condition in which the physician was when he had no recourse save to deal with health as entity or force on one side, and disease as opposing agency or invading influence upon the other. The teacher reacts *en bloc*, in a gross wholesale way, to something which he takes in an equally undefined and total way in the child. It is the inability to regard, upon occasion, both himself and the child as just objects working upon each other in specific ways that compels him to resort to purely arbitrary

measures, to fall back upon mere routine traditions of school teaching, or to fly to the latest fad of pedagogical theorists—the latest panacea peddled out in school journals or teachers' institutes—just as the old physician relied upon his magic formula.

I repeat, it is the fundamental weakness of our teaching force to-day (putting aside teachers who are actually incompetent by reason either of wrong motives or inadequate preparation), that they react in gross to the child's exhibitions in gross without analyzing them into their detailed and constituent elements. If the child is angry, he is dealt with simply as an angry being; anger is an entity, a force, not a symptom. If a child is inattentive, this again is treated as a mere case of refusal to use the faculty or function of attention, of sheer unwillingness to act. Teachers tell you that a child is careless or inattentive in the same final way in which they would tell you that a piece of paper is white. It is just a fact, and that is all there is of it. Now it is only through some recognition of attention as a mechanism, some awareness of the interplay of sensations, images and motor impulses which constitute it as an objective fact that the teacher can deal effectively with attention as a function. And, of course, the same is true of memory, quick and useful observation, good judgment and all the other practical powers the teacher is attempting to cultivate.

Consideration of the abstract concepts of mechanism and personality is important. Too much preoccupation with them in a general fashion, however, without translation into relevant imagery of actual conditions is likely to give rise to unreal difficulties. The ethical personality does not go to school naked; it takes with it the body as the instrument through which all influences reach it, and through control of which its ideas are both elaborated and expressed. The teacher does not deal with personality at large, but as expressed in intellectual and practical impulses and habits. The ethical personality is not formed —it is forming. The teacher must provide stimuli leading to the equipment of personality with active habits and interests. When we consider the problem of forming habits and interests we find ourselves at once confronted with matters of this sort: What stimuli shall be presented to the sense organs

and how? What stable complexes of associations shall be organized? What motor impulses shall be evoked, and to what extent? How shall they be induced in such a way as to bring favorable stimuli under greater control, and to lessen the danger of excitation from undesirable stimuli? In a word, the teacher is dealing with the psychical factors that are concerned with furtherance of certain habits, and the inhibition of others—habits intellectual, habits emotional, habits in overt action.

Moreover, all the instruments and materials with which the teacher deals must be considered as psychical stimuli. Such consideration involves of necessity a knowledge of their reciprocal reactions—of what goes by the name of causal mechanism. The introduction of certain changes into a net-work of associations, the reinforcement of certain sensori-motor connections, the weakening or displacing of others—this is the psychological rendering of the greater part of the teacher's actual business. It is not that one teacher employs mechanical considerations, and that the other does not, appealing to higher ends; it is that one does not know his mechanism, and consequently acts servilely, superstitiously and blindly, while the other, knowing what he is about, acts freely, clearly and effectively.[1]

The same thing is true on the side of materials of instruction —the school studies. No amount of exaltation of teleological personality (however true, and however necessary the emphasis) can disguise from us the fact that instruction is an affair of bringing a child into intimate relations with concrete objects, positive facts, definite ideas and specific symbols. The symbols are objective things in arithmetic, reading and writing. The ideas are truths of history and of science. The facts are derived from such specific disciplines as geography and language, botany and astronomy. To suppose that by some influence of pure personality upon pure personality, conjoined with a knowledge of rules formulated by an educational theorist, an effective interplay of this body of physical and ideal objects with the life of the child can be effective, is, I submit, nothing but an appeal

[1] That some teachers get their psychology by instinct more effectively than others by any amount of reflective study may be unreservedly stated. It is not a question of manufacturing teachers, but of reinforcing and enlightening those who have a right to teach.

to magic, plus dependence upon servile routine. Symbols in reading and writing and number, are both in themselves, and in the way in which they stand for ideas, elements in a mechanism which has to be rendered operative within the child. To bring about this influence in the most helpful and economical way, in the most fruitful and liberating way, is absolutely impossible save as the teacher has some power to transmute symbols and contents into their working psychical equivalents: and save as he also has the power to see what it is in the child, as a psychical mechanism, that affords maximum leverage.

Probably I shall now hear that at present the danger is not of dealing with acts and persons in a gross, arbitrary way, but (so far as what is called new education is concerned) in treating the children too much as mechanism, and consequently seeking for all kinds of stimuli to stir and attract—that, in a word, the tendency to reduce instruction to a merely agreeable thing, weakening the child's personality and indulging his mere love of excitement and pleasure, is precisely the result of taking the psycho-mechanical point of view. I welcome the objection for it serves to clear up the precise point. It is through a partial and defective psychology that the teacher, in his reaction from dead routine and arbitrary moral and intellectual discipline, has substituted an appeal to the satisfaction of momentary impulse. It is not because the teacher has a knowledge of the psycho-physical mechanism, but because he has a partial knowledge of it. He has come to consciousness of certain sensations, and certain impulses, and of the ways in which these may be stimulated and directed, but he is in ignorance of the larger mechanism (just as a mechanism), and of the causal relations which subsist between the unknown part and the elements upon which he is playing. What is needed to correct his errors is not to inform him that he gets only misleading from taking the psychical point of view; but to reveal to him the scope and intricate interactions of the mechanism as a whole. Then he will realize that while he is gaining apparent efficacy in some superficial part of the mechanism, he is disarranging, dislocating and disintegrating much more fundamental factors in it. In a word he is operating not as a psychologist, but as a poor psychologist,

and the only cure for a partial psychology is a fuller one. He is gaining the momentary attention of the child through an appeal to pleasant color, or exciting tone, or agreeable association, but at the expense of isolating one cog and ratchet in the machinery, and making it operate independently of the rest. In theory, it is as possible to demonstrate this to a teacher, showing how the faulty method reacts unhappily into the personality, as it is to locate the points of wrong construction, and of ineffective transfer of energy in a physical apparatus.

This suggests the admission made by writers in many respects as far apart as Dr. Harris and Dr. Münsterberg—that scientific psychology is of use on the pathological side—where questions of 'physical and mental health' are concerned. But is there anything with which the teacher has concern that is not included in the ideal of physical and mental health? Does health define to us anything less than the teacher's whole end and aim? Where does pathology leave off in the scale and series of vicious aims and defective means? I see no line between the more obvious methods and materials which result in nervous irritation and fatigue; in weakening the power of vision, in establishing spinal curvatures; and others which, in more remote and subtle, but equally real ways, leave the child with, say, a muscular system which is only partially at the service of his ideas, with blocked and inert brain paths between eye and ear, and with a partial and disconnected development of the cerebral paths of visual imagery. What error in instruction is there which could not, with proper psychological theory, be stated in just such terms as these? A wrong method of teaching reading, wrong I mean in the full educational and ethical sense, is also a case of pathological use of the psychophysical mechanism. A method is ethically defective that, while giving the child a glibness in the mechanical facility of reading, leaves him at the mercy of suggestion and chance environment to decide whether he reads the 'yellow journal,' the trashy novel, or the literature which inspires and makes more valid his whole life. Is it any less certain that this failure on the ethical side is repeated in some lack of adequate growth and connection in the psychical and physiological factors involved?

If a knowledge of psychology is important to the teacher in the grosser and more overt cases of mental pathology is it not even more important in these hidden and indirect matters—just because they are less evident and more circuitous in their operation and manifestation?

The argument may be summarized by saying that there is controversy neither as to the ethical character of education, nor as to the abstraction which psychology performs in reducing personality to an object. The teacher is, indeed, a person occupied with other persons. He lives in a social sphere—he is a member and an organ of a social life. His aims are social aims; the development of individuals taking ever more responsible positions in a circle of social activities continually increasing in radius and in complexity. Whatever he as a teacher effectively does, he does as a person; and he does with and towards persons. His methods, like his aims, when actively in operation, are practical, are social, are ethical, are anything you please—save merely psychical. In comparison with this, the material and the data, the standpoint and the methods of psychology, are abstract. They transform specific acts and relations of individuals into a flow of processes in consciousness; and these processes can be adequately identified and related only through reference to a biological organism. I do not think there is danger of going too far in asserting the social and teleological nature of the work of the teacher; or in asserting the abstract and partial character of the mechanism into which the psychologist, as a psychologist, transmutes the play of vital values.

Does it follow from this that any attempt on the part of the teacher to perform this abstraction, to see the pupil as a mechanism, to define his own relations and that of the study taught in terms of causal influences acting upon this mechanism, are useless and harmful? On the face of it, I cannot understand the logic which says that because mechanism is mechanism, and because acts, aims, values are vital, therefore a statement in terms of one is alien to the comprehension and proper management of the other. Ends are not compromised when referred to the means necessary to realize them. Values do not cease to be values when they are minutely and accurately measured.

Acts are not destroyed when their operative machinery is made manifest. The statement of the disparity of mechanism and actual life, be it never so true, solves no problem. It is no distinction that may be used off-hand to decide the question of the relation of psychology to any form of practice. It is a valuable and necessary distinction; but it is only preliminary. The purport of our discussion has, indeed, led us strongly to suspect any ideal which exists purely at large, out of relation to machinery of execution, and equally a machinery that operates in no particular direction.

The proposition that a description and explanation of stones, iron and mortar, as an absolutely necessary causal nexus of mechanical conditions, makes the results of physical science unavailable for purposes of practical life, would hardly receive attention to-day. Every sky-scraper, every railway bridge is a refutation, compared with which oceans of talk are futile. One would not find it easy to stir up a problem even if he went on to include, in this same mechanical system, the steam derricks that hoist the stones and iron, and the muscles and nerves of architect, mason and steel worker. The simple fact is still too obvious: the more thorough-going and complete the mechanical and causal statement, the more controlled, the more economical are the discovery and realization of human aims. It is not in spite of, nor in neglect of, but because of the mechanical statement that human activity has been freed, and made effective in thousands of new practical directions, upon a scale and with a certainty hitherto undreamed of. Our discussion tends to suggest that we entertain a similar question regarding psychology only because we have as yet made so little headway—just because there is so little scientific control of our practice in these directions; that at bottom our difficulty is local and circumstantial, not intrinsic and doctrinal. If our teachers were trained as architects are trained, if our schools were actually managed on a psychological basis as great factories are run on the basis of chemical and physical science; if our psychology were sufficiently organized and coherent to give as adequate a mechanical statement of human nature as physics does of its material, we should never dream of discussing this question.

I cannot pass on from this phase of the discussion without at least incidental remark of the obverse side of the situation. The difficulties of psychological observation and interpretation are great enough in any case. We cannot afford to neglect any possible auxiliary. The great advantage of the psychophysical laboratory is paid for by certain obvious defects. The completer control of conditions, with resulting greater accuracy of determination, demands an isolation, a ruling out of the usual media of thought and action, which leads to a certain remoteness, and easily to a certain artificiality. When the result of laboratory experiment informs us, for example, that repetition is the chief factor influencing recall, we must bear in mind that the result is obtained with nonsense material—*i. e.*, by excluding the conditions of ordinary memory. The result is pertinent if we state it thus: The more we exclude the usual environmental adaptations of memory the greater importance attaches to sheer repetition. It is dubious (and probably perverse) if we say: Repetition is the prime influence in memory.

Now this illustrates a general principle. Unless our laboratory results are to give us artificialities, mere scientific curiosities, they must be subjected to interpretation by gradual reapproximation to conditions of life. The results may be very accurate, very definitive in form; but the task of re-viewing them so as to see their actual import is clearly one of great delicacy and liability to error. The laboratory, in a word, affords no final refuge that enables us to avoid the ordinary scientific difficulties of forming hypotheses, interpreting results, etc. In some sense (from the very accuracy and limitations of its results) it adds to our responsibilities in this direction. Now the school, for psychological purposes, stands in many respects midway between the extreme simplifications of the laboratory and the confused complexities of ordinary life. Its conditions are those of life at large; they are social and practical. But it approaches the laboratory in so far as the ends aimed at are reduced in number, are definite, and thus simplify the conditions; and their psychological phase is uppermost—the formation of habits of attention, observation, memory, etc.—while in ordinary life these are secondary and swallowed up.

If the biological and evolutionary attitude is right in looking at mind as fundamentally an instrument of adaptation, there are certainly advantages in any mode of approach which brings us near to its various adaptations while they are still forming, and under conditions selected with special reference to promoting these adaptations (or faculties). And this is precisely the situation we should have in a properly organized system of education. While the psychological theory would guide and illuminate the practice, acting upon the theory would immediately test it, and thus criticize it, bringing about its revision and growth. In the large and open sense of the words psychology becomes a working hypothesis, instruction is the experimental test and demonstration of the hypothesis; the result is both greater practical control and continued growth in theory.

II.

I must remind myself that my purpose does not conclude with a statement of the auxiliary relation of psychology to education; but that we are concerned with this as a type case of a wider problem—the relation of psychology to social practice in general. So far I have tried to show that it is not in spite of its statement of personal aims and social relations in terms of mechanism that psychology is useful, but because of this transformation and abstraction. Through reduction of ethical relations to presented objects we are enabled to get outside of the existing situation; to see it objectively, not merely in relation to our traditional habits, vague aspirations and capricious desires. We are able to see clearly the factors which shape it, and therefore to get an idea of how it may be modified. The assumption of an identical relationship of physics and psychology to practical life is justified. Our freedom of action comes through its statement in terms of necessity. By this translation our control is enlarged, our powers are directed, our energy conserved, our aims illuminated.

The school is an especially favorable place in which to study the availability of psychology for social practice; because in the school the formation of a certain type of social personality, with a certain attitude and equipment of working powers, is the ex-

press aim. In idea at least no other purpose restricts or compromises the dominance of the single purpose. Such is not the case in business, politics, and the professions. All these have upon their surface, taken directly, other ends to serve. In many instances these other aims are of far greater immediate importance; the ethical result is subordinate or even incidental. Yet as it profiteth a man nothing to gain the whole world and lose his own self, so indirectly and ultimately all these other social institutions must be judged by the contribution which they make to the value of human life. Other ends may be immediately uppermost, but these ends must in turn be means; they must subserve the interests of conscious life or else stand condemned.

In other words, the moment we apply an ethical standard to the consideration of social institutions, that moment they stand on exactly the same level as does the school, viz.: as organs for the increase in depth and area of the realized values of life. In both cases the statement of the mechanism, through which the ethical ends are realized, is not only permissible, but absolutely required. It is not merely incidentally, as a grateful addition to its normal task, that psychology serves us. The essential nature of the standpoint which calls it into existence, and of the abstraction which it performs, is to put in our possession the method by which values are introduced and effected in life. The statement of personality as an object; of social relations as a mechanism of stimuli and inhibitions, is precisely the statement of ends in terms of the method of their realization.

It is remarkable that men are so blind to the futility of a morality which merely blazons ideals, erects standards, asserts laws without finding in them any organic provision for their own realization. For ideals are held up to follow; standards are given to work by; laws are provided to guide action. The sole and only reason for their conscious moral statement is, in a word, that they may influence and direct conduct. If they cannot do this, not merely by accident, but of their own intrinsic nature, they are worse than inert. They are impudent impostors and logical self-contradictions.

When men derive their moral ideals and laws from custom, they also realize them through custom; but when they are in

any way divorced from habit and tradition, when they are consciously proclaimed, there must be some substitute for custom as an organ of execution. We must know the method of their operation and know it in detail. Otherwise the more earnestly we insist upon our categorical imperatives, and upon their supreme right of control, the more flagrantly helpless we are as to their actual domination. The fact that conscious, as distinct from customary, morality and psychology have had a historic parallel march, is just the concrete recognition of the necessary equivalence between ends consciously conceived, and interest in the means upon which the ends depend. We have the same reality stated twice over: once as value to be realized, and once as mechanism of realization. So long as custom reigns, as tradition prevails, so long as social values are determined by instinct and habit, there is no conscious question as to the method of their achievement, and hence no need of psychology. Social institutions work of their own inertia, they take the individual up into themselves and carry him along in their own sweep. The individual is dominated by the mass life of his group. Institutions and the customs attaching to them take care of society both as to its ideals and its methods. But when once the values come to consciousness, when once a Socrates insists upon the organic relation of a reflective life and morality, then the means, the machinery by which ethical ideals are projected and manifested, comes to consciousness also. Psychology must needs be born as soon as morality becomes reflective.

Moreover, psychology, as an account of the mechanism of workings of personality, is the only alternative to an arbitrary and class view of society, to an aristocratic view in the sense of restricting the realization of the full worth of life to a section of society. The growth of a psychology that, as applied to history and sociology, tries to state the interactions of groups of men in familiar psychical categories of stimulus and inhibition, is evidence that we are ceasing to take existing social forms as final and unquestioned. The application of psychology to social institutions is the only scientific way of dealing with their ethical values in their present unequal distribution, their haphazard execution and their thwarted development. It marks just the rec-

ognition of the principle of sufficient reason in the large matters of social life. It is the recognition that the existing order is determined neither by fate nor by chance, but is based on law and order, on a system of existing stimuli and modes of reaction, through knowledge of which we can modify the practical outcome. There is no logical alternative save either to recognize and search for the mechanism of the interplay of personalities that controls the existing distributions of values, or to accept as final a fixed hierarchy of persons in which the leaders assert, on no basis save their own supposed superior personality, certain ends and laws which the mass of men passively receive and imitate. The effort to apply psychology to social affairs means that the determination of ethical values lies not in any set or class, however superior, but in the workings of the social whole; that the explanation is found in the complex interactions and inter-relations which constitute this whole. To save personality in all, we must serve all alike—state the achievements of all in terms of mechanism, that is, of the exercise of reciprocal influence. To affirm personality independent of mechanism is to restrict its full meaning to a few, and to make its expression in the few irregular and arbitrary.

The anomaly in our present social life is obvious enough. With tremendous increase in control of nature, in ability to utilize nature for the indefinite extension and multiplication of commodities for human use and satisfaction, we find the actual realization of ends, the enjoyment of values growing unassured and precarious. At times it seems as if we were caught in a contradiction; the more we multiply means, the less certain and general is the use we are able to make of them. No wonder a Carlyle or a Ruskin puts our whole industrial civilization under a ban, while a Tolstoi proclaims a return to the desert. But the only way to see the situation steadily, and to see it as a whole, is to keep in mind that the entire problem is one of the development of science, and of its application to life. Our control of nature with the accompanying output of material commodities is the necessary result of the growth of physical science—of our ability to state things as interconnected parts of a mechanism. Physical science has for the time being far outrun psychical.

We have mastered the physical mechanism sufficiently to turn out possible goods; we have not gained a knowledge of the conditions through which possible values become actual in life, and so are still at the mercy of habit, of haphazard, and hence of force.

Psychology, after all, simply states the mechanism through which conscious value and meaning are introduced into human experience. As it makes its way, and is progressively applied to history and all the social sciences, we can anticipate no other outcome than increasing control in the ethical sphere—the nature and extent of which can be best judged by considering the revolution that has taken place in the control of physical nature through a knowledge of her order. Psychology will never provide ready-made materials and prescriptions for the ethical life, any more than physics dictates off-hand the steam-engine and the dynamo. But science, both physical and psychological, makes known the conditions upon which certain results depend, and therefore puts at the disposal of life a method for controlling them. Psychology will never tell us just what to do ethically, nor just how to do it. But it will afford us insight into the conditions which control the formation and execution of aims, and thus enable human effort to expend itself sanely, rationally and with assurance. We are not called upon to be either boasters or sentimentalists regarding the possibilities of our science. It is best, for the most part, that we should stick to our particular jobs of investigation and reflection as they come to us. But we certainly are entitled in this daily work to be sustained by the conviction that we are not working in indifference to or at cross-purposes with the practical strivings of our common humanity. The psychologist, in his most remote and technical occupation with mechanism, is contributing his bit to that ordered knowledge which alone enables mankind to secure a larger and to direct a more equal flow of values in life.

PROCEEDINGS OF THE EIGHTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, YALE UNIVERSITY, NEW HAVEN, CONN., DECEMBER, 1899.

Report of the Secretary for 1899.

The eighth annual meeting of the American Psychological Association was held at Yale University, New Haven, Conn., December 27, 28 and 29, 1899, in affiliation with the American Society of Naturalists. The President of the Association, Professor John Dewey, of Chicago, presided at the meeting, at the various sessions of which there were fifty-six members in attendance. The members of the Association were present at the discussion before the Naturalists on the afternoon of the 28th, in which Professor Joseph Jastrow represented the Psychologists, the subject being 'The Position that Universities should take in regard to Investigation.' The members also attended the annual dinner of the Affiliated Societies held at the New Haven House on the evening of the 28th as well as the general reception extended to the visiting societies by Yale University on the evening of the 27th.

Two business meetings were held at which the following was transacted: Election of officers for 1900: *President*, Professor Joseph Jastrow, University of Wisconsin; *Members of the Council, to serve for three years*, Professor George T. Ladd, Yale University, and Professor William L. Bryan, Indiana University.

The following new members were elected: Professor J. A. Bergström, Indiana University; Dr. Henry Davies, Yale University; Dr. G. V. N. Dearborn, Harvard University; Dr. George B. Germann, Columbia University; Professor G. W. A. Luckey, Columbia University; Professor Lillie J. Martin, Leland Stanford Jr. University; Professor E. B. McGilvary, Cornell University; Professor Alexander Meiklejohn, Brown University;

Dr. Max Meyer, Clark University; Dr. A. H. Pierce, Amherst College; Dr. S. H. Rowe, New Haven, Conn.; Professor Carl E. Seashore, University of Iowa; Dr. Ellen Bliss Talbot, Troy, N. Y.; Professor J. H. Tufts, University of Chicago; Mr. Clark Wissler, Columbia University; Dr. R. S. Woodworth, University and Bellevue Medical School, N. Y.

Professors Ladd and James were appointed delegates of the Association to the International Psychological Congress to be held in Paris in 1900, and power was given to the Council to appoint other delegates in its discretion.

The Council was also empowered to call a meeting of the Association in June, 1900, in connection with the meeting of the American Association for the Advancement of Science to be held in New York at that time.

Upon motion of Professor Jastrow it was voted that the Association looked with favor upon the proposition to hold the annual meeting of 1901 in Chicago.

Upon recommendation of the Council the Treasurer was directed to pay, from the funds of the Association, the sum of fifty dollars to Professors Bryan and Lindley, to be used in the continuation of their investigation of the mathematical prodigy now under their charge.

The question of the arrangement of the program of the annual meeting with respect to technical papers and consequent formation of sections was actively discussed, but no definite instructions were given to the Committee of Arrangements. The Council was empowered to appoint a secretary to take charge of the arrangement of papers for the philosophical section.

Owing to the length of the program, the committee had followed the plan of dividing into sections on the morning of the 28th and on the 29th.

REPORT OF THE TREASURER FOR 1899.

DR.

To balance at last meeting,	$800 88
Dues of members,	297 00
	$1097 88

CR.

By expenditures for		
Postage, telegrams, etc.,	$17 70	
Stationery,	15 40	
Printing, clerical work, etc.,	14 10	
Expenses of meeting,	9 30	
		$56 50
Balance on hand,		$1041 38

Audited by the Council and found correct.

LIVINGSTON FARRAND,
Secretary and Treasurer.

ABSTRACTS OF PAPERS.

Address of the President: *Psychology and Social Practice.* By JOHN DEWEY.

The speaker discussed the relation of psychology to education considered as a form of social practice with which psychology might be expected to have most immediate concern, and then generalized the results reached to draw certain conclusions regarding the general value of psychology as a method to be applied in social life. He first pointed out certain psychological presuppositions controlling existing educational theory and practice: (1) the assumption of identity between the mental attitude of the child and the adult, overlooking the growth of specialized habits and aims in the latter, and the subordination of other considerations to questions of growth in the child; (2) the assumption of difference where identity is found, viz.: the motives which direct attention and secure mental power. An insight into the true psychology of the case on the part of both educational theorists and school instructors was pointed out as necessary to any intelligent and effective reform.

The objection that psychology deals with its material as objective and as mechanism, while education is concerned with living personalities, was then taken up; the purport of the discussion was to show the necessity of the statement of life functions and attitudes in terms of objective mechanism for the purpose of effectively directing them. The conclusion was drawn that it was not in spite of, or by ignoring, mechanical state-

ment that the ethical aims of education are subserved, but because of it.

Taking this conclusion as typical it was then argued that wherever it is a problem of reaching certain ends (that is wherever morality becomes consciously reflective) there is a demand for knowing the mechanism or method by which the end may be reached; that accordingly a reflective, as distinct from a customary, morality, and an attempt to get a psychological statement, were born and have grown *pari passu*.

(The address appears in full in the March number of the PSYCHOLOGICAL REVIEW.)

Discussion: *How Should Psychology be Taught?* The discussion was formally opened by PROFESSORS FULLERTON, JASTROW, AIKINS and JUDD, and continued by a number of members from the floor.

By PROFESSOR FULLERTON.

Professor Fullerton stated that he would make no attempt to cover the ground indicated by the title, and that he would not touch upon those pedagogical maxims common to psychology with many other disciplines, but that he would confine himself to one or two points of especial interest at the present time, owing to the developments of recent years. He spoke, in outline, as follows:

1. Certainly one of the most important questions which can face the teacher of psychology at the present day is the question of the adjustment of the relative claims of what have been somewhat unfortunately termed the New Psychology and the Old, the psychology of the laboratory and the statistical circular, and that which depends largely upon introspection and reflective analysis. There appears to be some danger that the one or the other may be unduly emphasized, when it is desirable that each should receive its due.

It may be impossible, in any given case, to give to the student even a brief course in the psychological laboratory. But this should be omitted only from stern necessity. The laboratory (*a*) awakens in the student's mind a realization of the

fact that psychology really is a natural science, and it yields him at least *some* exact information; (*b*) it gives the student a *standard of exactitude*, and he may hold this before him as an ideal even in those parts of psychology where the attainment of such an ideal is at present out of the question; (*c*) it helps to free the student from the bondage to tradition, which perpetuates error even when the means for its correction are not beyond our reach.

But it appears to be easy for some to overlook the fact that a large part of psychological doctrine is at present neither physiological nor experimental. This is clear to any one who examines with a critical eye the current systematic treatises on psychology—many of them written by men who have been trained in the laboratory. A large part of their contents have little direct connection with laboratory work, and rest upon the methods employed by what has been called the Old Psychology. The laboratory worker may or may not be a good man to teach this part of psychological doctrine, or, for that matter, to publish books upon it. It is an error to suppose that the whole of psychology should be relegated to the man in the laboratory and regarded as peculiarly his property. In the teaching of psychology emphasis should be laid upon this part of the science, as well as upon that which lends itself to exact experiment. There seems to be more danger of such one-sidedness here in America, where specialization in psychology has been pushed to its extreme limit, than there is elsewhere.

2. A second point of a good deal of living interest is the question whether the teacher of psychology—here it is chiefly the graduate instruction that is concerned—should devote his energies chiefly to enabling the student to publish some creditable piece of original work by the time he is ready to finish his course, or should make it his first effort to give him a thorough knowledge of the field as a whole, relegating to a second place the original work to be done in this or that part of the field. Something may be said on both sides of the question, but, in view of the present trend of public opinion, it may be well to point out the danger of excessive specialization. Some kinds of original work are in their nature broadening, but some are not.

A student may do some very useful pieces of work, and spend a disproportionate time in doing them, without being himself much profited by the work he has done. He may, in this case, finish his course, and find a position as a teacher when he is quite unfit to fill such a position creditably to himself or with advantage to the institution to which he goes. It is difficult to lay down any general rule, but the matter is one deserving attention, and a more thoughtful attention than it at present receives in some quarters.

By Professor Jastrow.

Professor Jastrow expressed the view that the discussion of 'How psychology should be taught,' was inseparably linked with that of 'What shall be taught?' and this in turn as a practical problem referred mainly to what shall be taught to the large introductory classes in colleges and universities. In answer to the practical aspect of the latter question Professor Jastrow laid fundamental emphasis upon bringing before the student such problems, for the data and principles of which he could be referred to the analysis of his own experience; the student must be infused with the conviction that the study of psychology does not involve altogether novel and abstruse material and strange methods of regarding this material, but that its comprehension depends intimately and constantly upon a common sense interpretation of daily and verifiable experiences. It is concerned with a systematic explanation of the student's mental functions; and this functional aspect of psychological problems is the one which, in the speaker's opinion, should dominate the manner of presenting psychology to the student. The one topic which seems pedagogically best suited to this end is the study of sense-experience, and the manner of deriving from it the information which all normal individuals possess. An essential and considerable part of an introductory course in psychology would consist of a series of demonstrations and explanations of sensory functions, of the kind of information which each of the special senses yields, and of the manner in which such information is obtained. Of other 'functional' problems coördinate in importance with this may be mentioned the general relations of

the constituent parts of conduct—the receptive processes, reflection, and coördination and movements or action—to one another and to the nervous system; the various degrees and kind of such conduct-arcs from the reflex to those of an elaborate voluntary type and so on. Regarding this 'functional' aspect as of greatest value in the teaching of psychology it follows that methods of greatest practical effectiveness would be sought and developed in the presentation of these topics. For most classes the demonstrational method seems both expedient and successful, but also demands the usual educational methods in vogue in introductory courses.

Supplementary to this and following upon it should be presented a clear and concise review of those aspects of mental experience which can best be studied by the light of introspection; and it should be made evident that many of the data utilized in the exposition of 'functional' principles were largely dependent upon introspective evidence. In this connection the broad field of psychological phenomena, their connection with probblems in cognate fields would naturally be presented, and an incentive be given for the student to pursue his studies along any of the lines which has appealed most strongly to his interests. And it would be well for both teacher and taught to feel and reflect as little as possible of the differences between what are misleadingly called 'experimental' and 'introspective' psychology, but to infuse the teaching with the spirit of solidarity and significance of the knowledge of mental phenomena.

By Professor Aikins.

Professor Aikins spoke in part as follows: My problem has been the practical one of constructing an elementary course that correlates text-book and experiment; that moves gradually from a familiar outward attitude towards mental facts and rough physiological explanations to introspective analysis and the more speculative problems; and that is concrete enough in the main to interest the average non-metaphysical student. The method I have finally adopted to solve my problem is this:

I begin with a general explanation of the nature of the reflex arc, and then take up the localization of function in the cord

and brain. I show how this proves the complexity of ordinary perceptions and thoughts, how it disproves the common absolute distinction between perception, memory and imagination, and so on; but what I dwell on most is the question why a neural discharge should travel to one center rather than to another and the conditions that determine its strength and duration.

Here comes in the experimental work. It is based upon the collection of experiments made by Professor Sanford; but I have tried to re-classify them according to the general tendencies of nervous action which they seemed to indicate or exemplify, and I succeeded in making a place for most of them under one or other of the following heads: Law of Diffusion, Law of the Threshold, Summation of Successive Stimuli, Reinforcement of Different Discharges by Each Other, Persistence of Neural Processes, Inhibition, the Effects of Novelty and Habit, the Law of Sudden Attainment, the Law of Dissociation or Forgetting, Fatigue, Rhythmic Functioning, the Order of Development, Brain Processes and Consciousness. These general tendencies or empirical laws of neural action I put together in a typewritten syllabus, and afterwards more fully in lectures, in each case stating the law and explaining its existence as well as I can and then giving examples arranged so as to show the extent of the tendency.

Having gone over these laws I take up all the topics in the text-book which can be treated from the standpoint of the reflex arc, and try to show in each case how the laws help to determine the form that the arc will take. Even Will I treat here as growing out of a conflict between several incompatible tendencies of the reflex type.

In the second part of the course I go over the ground again from the introspective standpoint, point out the aspects of mental life which no laws of nervous action can explain, and outline the historical speculative problems.

Such an arrangement of the work seems to me logical; it covers the ground fairly well; and with my students I think it works better than any other that I have tried.

By PROFESSOR JUDD.

Professor Judd held that the student finds in psychology certain difficulties which do not appear in his study of the other sciences. In physics, for example, the transition from observed facts to inferred interpretations is not so distinctly marked as to attract the attention of the student and impress upon him the indirect character of all inference. In psychology, on the other hand, because observation deals with the directly known facts of immediate experience, while inference always extends beyond the sphere of directly observed facts, the student becomes clearly conscious of the difference between observation and interpretation. He is frequently led to question the inferred facts because of the indirect method of arriving at them. This difficulty will be avoided if the first part of the course in psychology consists of a study of the indirect modes of treating subjective experience, that is of a study of the physical and physiological conditions of mental life. Such a study of conditions must not, however, be allowed to become pure physiology; it should always keep clearly in view its relation to the final treatment of mental experience towards which it is aiming. And the course should never be allowed to come to a close until the study of conditions with which it started has been extended so as to give a systematic view of the descriptive phases of psychological science as well as a view of the interpretation of psychical facts in their relations.

Volition as a Scientific Datum. By EDWARD FRANKLIN BUCHNER.

The paper discussed a tendency which characterizes current psychology, to rule out the volitional process. It presented an analysis of volition which entitles it to be placed among 'primitive' processes, while maintaining that it never is given as a pure process, but it is always found in constant relation with all contents of consciousness. Thus volition is ready and able to explain, in a psychological manner, rather than in need of explanation itself. To eliminate volition would be a virtual subversion of scientific psychology, so long as it is developed with the intent of representing the nature of mental activity and the course of its development.

The Criterion of Sensation. By GEORGE STUART FULLERTON.

This paper was a continuation of the train of thought developed in a paper entitled 'The Psychological Standpoint,' read before a former meeting of the association, and which endeavored to show what is implied in the recognition of psychology as a natural science.

The present paper treated the same theme in a more limited field, maintaining that the criterion of sensation offered by psychology as natural science—the reference to the peripheral nervous system—is one which can be regarded as ultimate only for convenience of discussion within a definite field of investigation; that the psychologist must ultimately rest upon the criterion recognized as final by the epistemologist; and that the contradiction in the psychological standpoint, the quarrel between the doctrine of representative perception and the assumption that we immediately perceive a real world, is eliminated when one recognizes the fact that, although the distinction between things and the percepts through which they are known is final for psychology as natural science, it is not in the same sense final for epistemology. It was maintained that the psychologist is quite justified in assuming that he immediately perceives a world of real things, and that he may begin with this initial assumption in building up his system of doctrine. But it was further maintained that he must hold faithfully to his doctrine of representative perception, and not endeavor to render his procedure consistent by asserting that the mind immediately perceives things and percepts alongside of each other, a doctrine which is subversive of the very foundations of psychology when carried out in its consequences. The psychologist should, it was held, leave his defense to the epistemologist, and be content with an inconsistency which works no harm in practice, and which is an inconsistency, after all, only when looked at from the plane of the common understanding.

(The paper is printed at length in the PSYCHOLOGICAL REVIEW for March, 1900.)

An Arithmetical Prodigy. By E. H. Lindley and W. L. Bryan.

Arthur Griffith, born 1880, at Milford, Indiana, son of a stone-mason; had a passion for counting from the age of three; entered school at the age of ten; attended school seven years; made a fair record in all studies; came to Indiana University to be investigated November, 1899.

Investigation covered following points: (1) general physical and mental organization; (2) sensory and motor capacities; (3) memory and type of imagination; (4) rapidity of calculation; (5) conditions of rapidity; (6) origin and significance of his methods.

The principal results are as follows: In scope and tenacity of memory and in rapidity he ranks among the best recorded cases. He is unique in the large number of methods which he has worked out and in the fact that he explains how and when he arrived at these. His memory is of the systematic type. His rapidity is found to depend upon the great number of number relations committed to memory, and upon the reduction in the number of operations through short-cut methods.

The boy gave demonstrations before the Association on Wednesday and Thursday.

Moral Perceptions of School Children. By Will S. Monroe.

The speaker traced the development of the moral sense from the spontaneous outgoings of sympathy for animals to its higher reaches of sympathy for human beings. His thesis was illustrated by the interest of a set of school children in a tame crow. Sympathy was the dominant characteristic of the study, and this real and profound sympathy for the crow he thought unmistakable evidence of the development of the moral sense, growing out of a vague gregarious instinct, doubtless, but nevertheless training the moral sense of these young people for adjustments to human relations in the larger experience of the later life.

Individual Tests of School Children. By E. A. Kirkpatrick.

Children of all grades from the first to the eighth were tested: (1) as to rate of counting, (2) rate of making vertical

marks, (3) sorting cards according to oral direction, (4) sorting cards according to visual direction and (5) tendency to see ink spots as objects. The results showed general improvement in the first four tests up to about the sixth or seventh grade, when the climax was reached. In the fifth test the children were best in the first grade and poorest in the fourth, fifth and sixth.

These results taken in connection with those obtained by others and interpreted in the light of the laws of decreased effects of practice as the limit of improvement is approached, and the theory of child development that some powers naturally mature before others, indicate that tests of individual ability cannot be properly interpreted till we know what mental activities are naturally most prominent at the age of the person tested, for a high record in a test of powers naturally developed earlier, may indicate that the development of higher powers has been arrested while elementary ones have continued to improve. It is therefore necessary to devise tests that can be given not only to university students, but also to children of all ages before we can properly interpret the results of tests of university students.

The children of each grade were arranged in three groups according to ability by their teachers and by their record in these tests, with the result that for 57 per cent. the two groupings agreed, while only 2 per cent. differed by two points. Such tests would probably be valuable in doubtful cases of promotion and classification.

The Reliability of Certain Methods for Measuring the Degree of Fatigue in School Children. By T. L. BOLTON.

The method of Griesbach and the method of the ergograph are under consideration. Do the sensory circles vary directly with the degree of fatigue as Griesbach affirms? And can all reductions in muscular power be referred back to fatigue for a cause or will fatigue be followed always by reductions in muscular power? Can the æsthesiometer and the ergograph be used to diagnose the fatigue condition of school children? Is it possible, as the followers of Griesbach have tried to do with the æsthesiometer, and Kemsies with the ergograph, to deter-

mine the fatigue value of each subject of the school curriculum? Determinations of the space-threshold were made before and after different periods of severe mental work, the work beginning always at the same time of day. The method of minimal changes was employed to make the determinations of the space-threshold. In the first series of experiments very slight increase was observed in the space-threshold after mental work, but in a subsequent series of experiments made under the same circumstances this increase in the space-threshold disappeared. In a third series of experiments for fifteen days, determinations of the space-threshold, combined with ergograph test, were made before and after two-hour periods of mental—adding—work, five two-hour periods of walking and five two-hour periods of resting. The purpose was to test the different influences of mental work, bodily exercise and rest. The space-threshold was found not to vary regularly under any of these influences. The performances with the ergograph were considerably increased after mental work, decreased to an equal amount by two hours' bodily exercise and remained practically unchanged after a two-hour period of rest.

The conclusions were that severe mental work for two hours does not affect the space-threshold sufficiently to be detected by the æsthesiometer, that this instrument is not adapted to the diagnosis of fatigue conditions in school children, that all reductions in the power of muscular endurance are not significant of fatigue, and that this instrument also is not adapted to measuring the degree of fatigue in school children.

Preliminary Report on Some Tests of Individual Sense Types. By HOWARD C. WARREN. (Read by title.)

A New Number Form. By EDWARD FRANKLIN BUCHNER.

This contribution presented a description, accompanied with charts, of tri-dimensional forms for numbers from 1 to 100, for the days of the week, and for the months of the year, possessed since her early childhood by a woman thirty-six years of age. No explanation for the genesis of the forms was derived from their history.

On the Time-values of Accented and Unaccented Elements in Rhythm. By ROBERT MACDOUGALL.

A Method of Securing Enlarged Records of Voice Vibrations. By CHARLES H. JUDD.

This paper reported a simple, direct method of securing on smoked paper enlarged records of voice vibrations together with a parallel time record. The diaphragm used in this method consists of thin tracing paper drawn tightly over the end of a shallow wooden speaking tube. The central area of the diaphragm is rendered rigid by pasting a small disk of cardboard to the paper. This central rigid area serves to concentrate at the center all the energy expended upon it, thus giving vibrations strong enough to move the levers. The recording lever which is connected with this diaphragm is horizontal in position and is made of straw. The greatest possible freedom of movement and the least possible weight are thus secured. As a test for the efficiency of this diaphragm and lever a string was connected with the recording arm, not, it will be noted, with the diaphragm, and led to a second diaphragm. Whatever vibrations are produced in this second diaphragm will be recorded on the smoked paper. It was found that the second diaphragm reproduced clearly the articulations received at the first diaphragm. Records made by the method described were exhibited and the analysis of a four-syllable word was reported in detail.

Researches in Experimental Phonetics. By E. W. SCRIPTURE.

Elements of a Psychological Theory of Music. By MAX MEYER.

The present theory of music is based on the diatonic scale of seven notes represented by the relations 24, 27, 30, 32, 36, 40, 45. The other tones are considered as intercalated tones, bearing no relation to the scale. In the present theory only 2, 3 and 5 relations are considered as harmonious; the 7 relations are not so considered.

In order to perfect the theory of music we utilize the 7 relations. The mind does not grasp relations more distant than 7

relations, hence these may be disregarded. Besides insisting upon the use of the 7 relations, we divide all tone groups into two classes: groups containing the 2 relation, and groups made up of the 3, 5 and 7 relations, the 2 relation being omitted. We thus make clear many of the æsthetic effects of music.

Is There an Independent Auditory Space? By A. H. PIERCE.

The peculiar structure of the sense-organ of hearing, and the inevitable fact that auditory spatial determinations are subordinated to those of sight and touch, have led the majority of psychologists to deny the possibility of an independent auditory space. It is claimed that all apparently spatial auditory experiences are really accomplished by a systematic *borrowing* from the visual and tactual fields. Thus localizations of sound are said to be 'transferred' localizations.

An independent auditory space could be posited with assurance if definite localizations of sound could be shown to take place under circumstances which absolutely preclude the coöperation of visual, tactual and motor factors. Now, just these conditions are fulfilled by the so-called 'intracranial localizations.' Such localizations occur when two fusing sounds are given simultaneously, one at each ear and in close proximity to the head. These are genuine auditory phenomena, and the spatial determinations to the interior of the head are as immediately and confidently accomplished as when the sound is located externally. Here then we have facts that are decisive in favor of an independent auditory space. For these localizations cannot have been made by the aid of factors borrowed from the other space fields.

Still, though we may believe that an auditory space with its peculiar laws and apparent caprices is no fictitious affair, we must freely confess that it must forever remain in a subordinate position among the spaces. For there are no extended auditory objects, and auditory localizations are too liable to be faulty to satisfy the demands of daily life.

Some Experiments on Motor Diffusion. By CLARK WISSLER.

Some experiments were reported showing the time relations

between the primary contractions of finger muscles and the accompanying secondary contractions of the other arm muscles. By primary contractions are meant those intentionally made, and by secondary those accompanying unintentional movements due to a diffusion, or spread, of the motor discharge. The time relations were measured by graphic means. The results lead to the conclusion that in case of arm muscles the primary contractions are first in order of time and followed by secondary contractions in an order corresponding to their distance anatomically from the muscle innervated. This time order taken in connection with the fact that secondary contractions occur on the other side of the body leads to the view that this phenomenon is the result of an irradiation of the discharge from the primary center in the cortex. That this diffusion takes place in both directions is indicated by the fact that training the muscles of the fingers also trains the other arm muscles and training the biceps in turn trains the finger muscles. From this point of view transference of practice effect is simply the result of diffused nerve currents.

(Printed in full in the PSYCHOLOGICAL REVIEW for January, 1900.)

The Influence of Special Training on General Ability. By E. L. THORNDIKE and R. S. WOODWORTH.

Dr. Thorndike and Dr. Woodworth reported the results of some experiments seeking to determine the influence of special training on general ability. The abilities studied were (1) the speed and accuracy of making certain complex observations, *e. g.* of picking out from a page of print all the verbs or all the words containing both *r* and *e*; (2) the recognition of weights, lengths and sizes; (3) attention to and retention of names; (4) discrimination of two complex objects shown successively.

In the first of these experiments subjects were given a test of ability to recognize accurately a number of different combinations of letters before and after a period of practice in recognizing some one combination.

In the second series of experiments different sense judgments were studied by a method somewhat resembling that of

average error. The preliminary test consisted, for instance, in the estimation of the absolute lengths of lines 6–12 inches long. The subject was then given a set of cards on which were lines ranging from ½–1½ inches, and trained himself by correcting his errors until he could tell the length of any such line to an eighth of an inch, with very few mistakes. He was now re-tested in estimating the longer lines. A similar method was employed in the perception of areas, weights, etc.

The third and fourth series of experiments were class-tests tried on first- and second-year medical students. The special training was in this case supplied by the first year's work of the medical course. Since this year introduces the student to a vast amount of practice in learning names and in observation along the lines of anatomy, histology, chemistry, etc., it was argued that if any special training could improve the general powers of retention of names and of observation, this year's work should do so.

These experiments unanimously failed to detect any pronounced influence of special training on general ability, but are not conclusive.

Unsatisfactory Elements in Current Views on Light and Color. By A. KIRSCHMANN. (Read by title.)

On Relations of Time and Space in Vision. By J. MCKEEN CATTELL.

When a moving surface is exhibited as it passes under a window in a screen it appears larger than the window. If green is exhibited first for $\frac{1}{20}$ second, followed by red for $\frac{1}{20}$ second, the observer does not see green followed by red, but the two colors are seen side by side, variously arranged and intermingled, filling a larger area than the window through which they are seen. A series of physical and physiological processes in time makes for perception a spatial continuum. In this case, however, the same physical stimulus gives rise to entirely different perceptions with different observers, indicating that the processes of visual perception are largely built up by the individual.

When in the ordinary vision of daily life the line of sight

moves over objects, say the books on a shelf, each retinal element is successively stimulated, but the objects are seen simultaneously, side by side. In this case the intermittent stimulations may occur as rapidly as 1,000 per second without any fusion or blurring. This fact indicates that fusion and, indeed, all the phenomena of color-vision are cerebral rather than retinal.

These experiments, demonstrating as they do that a time series is perceived as a space continuum when this is advantageous for our reactions, show anew that our perceptions are not 'copies' of a physical world or correlates of simple physiological processes, but are dependent on experience and utility.

An Effect of Eye Movements in Visual Perception of Space. By C. E. SEASHORE. (Read by title.)

Conditions Affecting the Judgment of the Direction of Lines. By E. D. DELABARRE.

In the judgment of the vertical, beside the factors usually recognized, the following are of especial importance: (1) attention-fixation does not usually coincide with eye-fixation, though the observer ordinarily assumes that it does; instead, the eye-fixation wanders much unconsciously on either side of the supposedly fixated line, and causes it to appear constantly changing in degree and direction of inclination; (2) muscle-strains in head and eye also influence the apparent inclination.

These same influences affect also other judgments of direction, and likewise judgments of length and distance. Certain conditions—of illumination, attracting points in the field, and arrangement of lines—produce definite strains and tendencies to fixation of particular kinds. These furnish a fundamental explanation for many forms of geometrical-optical illusion.

Recent Studies in the Clark Laboratory. By E. C. SANFORD.

Dr. Sanford reported briefly upon three studies with the following tit es:

1. The Development in School Children of the Ability to Reproduce Rhythms.

2. The Rhythm of Nursery Rhymes.

3. The Mental Process of the White Rat as Tested with the Maze.

The first and last of these are portions of more extended studies of the general topics of rhythm and comparative psychology, and all are expected to appear in the *American Journal of Psychology* during the coming year.

Pending Problems at the Wisconsin Laboratory, with Demonstration of Some Optical and Other Apparatus. By JOSEPH JASTROW.

Professor Jastrow demonstrated various pieces of apparatus, many of them connected with the study of problems in visual perception. The Wood pseudoscope, and the reflecting stereoscope, which also enables one to combine ordinary stereoscopic views either stereoscopically or pseudoscopically (see PSYCHOLOGICAL REVIEW, January, 1900), were exhibited and their modus operandi explained. A device for simplifying the demonstration of retinal shadows (Le Conte's experiment) was shown. Attention was also called to the fact that red and green spectacles and appropriate red-green prints, for stereoscopic vision, were now commercially available; and that a set of mathematical curves in red-and-green stereoscopy, better suited for demonstrations than miscellaneous prints, was about to be published. The demonstration further included a brief account of experiments in progress on the power of distinguishing in a shadowless light between convex forms varying slightly and regularly in degree of convexity; and of a simple reconstruction of a typewriter by means of which this instrument could be made to serve a variety of psychological purposes, most of them connected with the study of practice and the acquisition of efficiency. The sorting apparatus (PSYCHOLOGICAL REVIEW, May, 1898) was exhibited in its perfected and portable form.

Demonstration of New Apparatus. By E. W. SCRIPTURE.

Physical Psychology. By A. H. LLOYD. (Read by title.)

The 'Natural History' Point of View in Psychology. By E. H. GRIFFIN.

Can the differentiation of psychology from ultimate inquiries be carried to the extent of excluding all postulates that may be characterized as metaphysical?

The analogy of other natural sciences is incomplete, since they disregard presentation in consciousness. They abstract their material from the knowing subject, and treat it as independently given, whereas psychology takes as its special theme the subjective aspect which the other sciences neglect. It is not difficult for the physicist or the chemist to avoid raising any question as to the nature of being, or the basis of knowledge, but such questions are obtruded upon the psychologist because the essence of his facts is their character as modifications of a conscious self. Many important phases of psychical experience must be left unexplained, if the dictum, "Let psychology treat its material as the other sciences treat theirs," is strictly adhered to. Perception must be handed over to the epistemologist. The psychologist may describe, and analyze, and determine the genetic conditions of the states of consciousness which we call perception, but he cannot deal with their cognitive value as purporting to reveal an external world. The validity of reasoned knowledge also lies beyond his province; to establish the veracity of the reasoning function the resources of another science must be invoked. The continuity of experience in memory cannot be discussed in terms of a merely descriptive psychology. The will must be reduced to a complex of sensations, if, in this sense of the word science, it is to be scientifically treated. The title of Professor Münsterberg's striking article, 'Psychology and the Real Life' (*Atlantic Monthly*, May, 1898), suggests the fundamental criticism that may be made upon the 'natural science' point of view when illegitimately extended. The will analyzed by the psychologist is not, we are told, the real will, but is an artificial abstraction from it containing only its sensational elements. Is an abstraction that is distinctly misleading justifiable? When an investigator isolates a certain property from others and treats it as independent, or employs a symbol, or constructs a hypothetical entity like the other, these devices are not supposed to misrepresent the facts which they set forth.

The exclusion of epistemology and metaphysics from psychology is, within certain limits, desirable. It is evidently expedient to set apart certain psychophysical phenomena for treat-

ment after the methods of empirical science. But the ontological implications of experience must be acknowledged by the psychologist as soon as he passes beyond what may be called the sensational life. Perception, and memory, and reasoning, and attention—not to speak of the æsthetical and ethical emotions, and the higher determinations of choice—cannot be comprehended from a merely psychophysical standpoint.

Kant's Doctrine of Apperception and the Categories. By J. H. HYSLOP.

The main object of this paper was to show that Kant's doctrine of apperception contained a variety of processes which ought to be distinguished from each other. In order to indicate this fact a criticism of the Kantian categories was presented with a view of showing that those of relation and modality had been excluded from formal logic and only those of quantity and quality retained. The effect of this is to make the problem of knowledge extra-logical and to necessitate a recasting of both the problem of judgment and the categories. As a venture, therefore, in regard to both these questions the paper endeavored to indicate merely that the categories, in so far as the main question of content as distinct from conviction is concerned, might be reduced to one type, namely, that of Sufficient Reason, which was subdivided into the *Ratio Fiendi* and the *Ratio Essendi*, the former that of efficient and the latter of material cause. These were then shown to determine the contents of the two fundamental types of judgment to which all forms of judgment were reducible, namely, the intensive and the extensive judgment. The intensive judgment has to do with the relation between substance and attribute, and the extensive the relation between species and genus. The first of these applies the category of causality in some form and the latter that of comparison, or identity and difference.

Kant's doctrine of apperception was shown to comprehend too many processes to be as simple as it was supposed, and on that account gets into the difficulties of affirming the *Ding an sich* and yet not needing it for the extensive judgment which for Kant practically exhausted the problem of knowledge, though

he did not originally intend that it should be so. Consequently, by distinguishing between the *universal* and the *singular* judgment in the problem of knowledge and also between the *ordo cognitionis* and the *ordo naturæ*, we can start with the predicate in the intensive judgment and obtain a subject through the category of causality by an immediate process which does not assert more than the necessity of a subject to account for the predicate as a phenomenon, leaving all questions as to farther meaning either unsolved or open to later discovery. This process is one that excludes the application of the principle of identity from the determination of the primary judgment in knowledge.

Pragmatism. By WILLIAM CALDWELL.

Metaphysical Method. By J. A. LEIGHTON.

The problem of metaphysics is to discover a final ground for the relation assumed to exist in theory and practice between the finite subject and the rational objective order which the conscious activities of the subject, both theoretical and practical, imply. The method must recognize that thought and will are complementary and inseparable phases of the subject's activity and that the relation of the latter to the objective order is a developing one. While, before Kant, analysis and synthesis were both used, they were not combined in a living unity but, under the influence of mathematics, stood only in an external relation to one another. The method sees analysis and synthesis as the complementary aspects of a dynamic relation of the subject to the objective order. Hence the true method will find the metaphysical ground in the *life-history* of the finite consciousness as the latter develops by an immanent process in relation to an objective order, which while it responds to the movement of the finite subject and is in dynamic relation to the latter, nevertheless, as the organic wholeness of finite subjects, is forever a complete unity. The method recognizes in art, morality and religion relatively full experiences of this unity, but shows that these immediate experiences can be understood only in relation to the dialectic development of the self which has them.

The Concept of Substance. By ALEX. MEIKLEJOHN.

The purpose of this paper is to define the term 'substance' as used by Aristotle and Kant. For the former, a substance is an 'object of predication'; for the latter it is also a 'permanent object.' The task then is to define the thought, 'a permanent object of predication.' The concept cannot be abolished from thought, nor can it be declared indefinable. As Aristotle tells us, without this notion no predication is possible, and hence the interests of clear thinking demand that we know what our own term means. Locke's analysis of the concept is destroyed by Hume; and, further, his assertion that an object is unknown when its qualities are known is evidently false, as an account of the relation of substance and quality. The view which identifies substance with 'essence' or 'essential nature' is also untenable. To hold it is to declare objects identical in 'essence' to be identical as substances, *i. e.*, as individual objects. The truth is that the thought 'substance' is not qualitative or descriptive, but existential and demonstrative. In Aristotle's phrase 'this particular object' there is no description of the object, but only a singling of it out as 'this existent.' And when we regard the object as permanent we do not mean that it is unchanged in nature, but that it is unchanged in 'separate existence'; it is still 'this existent' whenever then we single out an object of attention of which to affirm qualities and change of qualities, together with the thought of qualities is the existential and demonstrative element, which constitutes our notion of substance.

The Influence of Philosophers on Public Opinion. By ALBERT SCHINZ. (Read by title.)

Practical Procedure in Inference. By JOHN GRIER HIBBEN.

In the various processes of actual reasoning we find a departure from the traditional syllogistic form, and that in several particulars.

1. There is usually a suppressed premise. The term euthymeme is misleading, implying that the omitted premise is expressed in thought though not in words, whereas the question

suggests itself whether the omission in language does not indicate a like abbreviation in thought.

2. A syllogism often contains four or more terms, when, for instance, a term is repeated in the form of its contradictory.

3. There may be a syllogistic form, and yet the criticism of the inference does not turn upon the rules concerning distribution of terms.

4. A formal criticism is often to be supplemented by material considerations. In actual inference the procedure is more direct and simpler than the formal syllogistic criticism. It retains the essentials of syllogistic inference, namely, an identical point of reference and a universal relation, and yet it need not give formal expression to them. This simpler method consists in a direct reference of the inferred element to its appropriate place in the system of knowledge by virtue of the canon of congruity, *i. e.*, that every inferred element must be congruent with the particular system to which it belongs and incongruent to all others.

There may be three modes of inference:

1. Given a subject, we infer its marks. This is the logic of analysis.

2. Given marks, we infer their appropriate subject. This is the logic of identification.

3. Given certain marks of a subject, we infer other marks necessarily growing out of the given. This is the logic of elaboration.

In all these forms the inferred elements are directly referred to their proper place in a particular system. The procedure is not from the known to the unknown; it is rather a revelation of that which is contained within the known, and enveloped by it. Some practical canons to guide in inference:

1. When, in a judgment expressed in propositional form, the predicate is *a* mark but not *the* exclusive mark of the subject, then the subject always implies predicate, but the predicate does not necessitate the subject.

2. When the predicate is an accidental mark of the subject, then no inference is possible.

3. When the predicate is recognized as the distinctive mark of the subject, then, given either subject or predicate, the other is implied necessarily.

Our practical inferences run along some such lines as above indicated. From a knowledge of the subject-predicate relations inference is simple and direct.

Elements of Consciousness. By MARY WHITON CALKINS.

This paper considered the principles of classification of elements of consciousness, conceived as absolutely unanalyzable. The author recognized three groups of such elements: (1) Sensational (or 'substantive') elements; (2) 'attributive' elements (including the affections 'pleasantness' and 'unpleasantness' and the 'feeling of realness' unless this last be a complex); (3) 'transitional' elements, such as the consciousness of 'oneness' and of 'manyness.'

Two portions only of the paper were read. The first of these proposed as a distinction between sensational qualities and sensational intensities the difference between simple qualitative series, like red, yellow, green, blue, in which the increase is of the *difference*, not of the sensational quality, and intensity series, like loud, more loud, still more loud, in which the increase is of the sensational intensity itself.

The second extract from the paper argued for the right of 'transitional' experiences to a place in any enumeration of psychic elements, on the ground that we habitually have experiences, like those of familiarity or of similarity, which simply are not reducible to complexes of sensation and affection. In anticipation of objections from both schools of psychology, it was insisted, on the one hand, that these 'transitional' elements are actual experiences, not logical categories; and, on the other hand, that the method of assuming contents of consciousness, without explicit reference to self-conscious selves, and of analyzing these contents into their elements, though not the only psychological method, is nevertheless an inclusive one, as applicable to what is called 'thought' as to 'perception,' so that the 'transitional' elements are as certain as the 'sensational' ones.

Nature and Choice. By EDGAR A. SINGER, JR.

The history of this problem reveals a certain change of attitude. From the primitive mind that finds nature full of caprice,

but sees its world in only one way, we pass to a science that strives to present nature in a 'single-valued function,' but recognizes the part played by the point of view in imposing such an interpretation on the facts. The one looks for choice within the world described and finds it in instances of lawlessness. The other looks for choice in the function of description and finds it in the formulation of law.

The paper proceeded along historical lines to show that science recognized a truth in its descriptive formulæ that was not merely conformity to fact. In its classifications and its choice of axioms it performed a function that was not uniquely determined by the facts described. But this choice, though not eliminated by observation, is not caprice. It must be exercised in a way that is determined by the meaning of 'experience' itself. To derive this principle of choice is to perform the old Kantian problem of 'deduction.' But whereas Kant arrived at the 'unity of apperception' as the contribution of the describer to the product we call experience, the post-Kantian thought has taught us to view experience in a more dynamic way. The thought's struggle after maximum unity might better represent the function which, from a study of the history of science, we should be led to regard as the describer's contribution. The liberty that the individual might enjoy to give up this struggle was not denied; but it was urged that experience was essentially a social product, and it was as a social, not as an individual function, that this principle was necessary to the meaning of a descriptive experience.

Methodology and Truth. By J. E. CREIGHTON.

The Spiritual Principle in T. H. Green's Philosophy. By E. B. MCGILVARY.

Green's whole procedure turns upon the assumption that consciousness cannot be developed by a process of nature. This would be true only if the natural process were a *mere* series, or if development demanded an abstract sameness through all its stages. Neither condition can be granted. Green's argument to prove that consciousness cannot result from change

rests upon the ambiguity of the word 'present.' Another fallacy is that of abstracting the object of *our* temporal consciousness from our consciousness, and then putting it in organic relation with an *eternal* consciousness.

The idea of eternity, however, cannot be dispensed with; but it needs to be redefined. We cannot follow Green in interpreting it as everlasting unchangingness through time. Eternity is transcendence of time, and there are three different ways in which time is transcended: 1. The organic totality of the universe, though a totality of parts in time, is not itself in time, inasmuch as it is neither before, nor after, nor simultaneous with, anything outside itself. This transcendence of time by the whole time-order is eternity. 2. Every time element in this order, though having a definite temporal position, has an influence over every other element at every other time, and this dynamic transcendence of time is eternity. 3. Knowledge, though an act in time, is not confined to its own time, in that it can present to itself the past and the future. This epistemological transcendence of time is eternity. These three eternities are organically related.

The Relation between the Natural Order and the Moral Order of the Universe. By DAVID IRONS.

Moral obligation is the form in which law appears in self-conscious beings who are no mere spectators of cosmic forces, but conscious combatants in the struggle. The law is internal; what it demands is felt as something we owe to ourselves. But while it is not an external force compelling us to act in one definite way, it is none the less inflexible. We must adopt a certain mode of behavior or be unworthy in our own eyes. From this alternative there is no escape. What we feel we owe to ourselves cannot be separated from our nature or from the circumstances in which we are placed. The content of the moral law in any particular case is determined by the nature of the individual and of his environment, *i. e.*, by the individual's place in the system of things. Hence, so far as it is realized, the moral order cannot be at variance with the natural or cosmic order, since its relation depends upon the fact that each individ-

ual performs his proper function. Immorality, departure from the moral order, is the only thing at variance with the order of the universe.

The Development of Content in Moral Judgments. By ELLEN BLISS TALBOT.

Moral judgment, according to Simmel's conception of it, has two elements, the matter (the idea of some particular action) and the form (the feeling of Ought). From the observed fact that the moral judgments of various ages and races differ so widely, Simmel infers that the Ought has no special content of its own, but can attach equally well to the ideas of all sorts of actions. It is not possible to disprove this theory of Simmel's, but it may be shown that another position is tenable. Simmel conceives human consciousness as a merely artificial union of form and content. In this respect he resembles Kant. Fichte and Hegel differ from Kant in regarding consciousness as a self-developing form, in which the content, at first implicit, is gradually becoming more explicit. If we view experience in this way, we shall explain the heterogeneity of moral judgments, not by saying that the Good is mere empty form, but by supposing that its content is not yet sufficiently explicated to be perfectly understood. This theory involves the doctrine that so long as there is moral progress, the nature of the Good cannot be fully known. On this view, the great diversity in ethical theories is easily explained.

The Relation of Ethics to Religion. By WALTER GOODNOW EVERETT.

The purpose of the paper is to inquire how the relation of ethics to religion must be conceived by one who seeks to subject the phenomena of man's moral life to scientific treatment. A distinction must, therefore, be made between the historical and the logical, the practical and the theoretical point of view.

1. It is necessary first to determine the generic character of the two classes of phenomena. Religion involves belief in a higher power or powers, in a transcendent being or beings, and also some form of cult or worship. Morality has to do with man in personal and social relations.

Religion and morality may also be distinguished as regards their source in man's nature. Religion has its source primarily in the relations which man sustains to nature, morality in the relations which he sustains to his fellows.

2. Religion and morality act and react upon each other. The moral attributes of deity are drawn from the ethical ideals of the chief worshippers. Man has projected upon the Infinite Spirit the highest excellence he has known. The ethical content of religion is due to an immanent, not a transcendent process of development.

3. Since almost every religion has taken up into itself some more or less detailed code of morals which it has imposed as matter of religious obligation, it is important to distinguish between a theological and a scientific system of ethics. The one rests upon religious presuppositions; its requirements are commands of the deity, its sanctions rest in his will. The other deals with discoverable facts of the present order, and its sanctions are found in the inescapable consequences of right and wrong conduct. But a metaphysic, a philosophy, of conduct, as distinguished from such a science, leads to the deepest problems of religion.

4. Three moments in the evolution of religion may be distinguished: (1) a stage of magic and sacrificial rites; (2) a stage at which creed and dogma are emphasized; (3) a stage at which the emphasis is shifted to morality. In a profoundly ethical religion like Christianity there result a unification and a harmony of the religious and moral life. Morality is warmed and brightened by faith in an Infinite Spirit, the creator and guarantor of the moral order.

5. In attempting to establish theoretically a religious view of the world one must proceed from the known to the unknown. Here ethics as a part of the known order comes first. How shall I interpret the world? By what it does. What then shall I think of a world-order which has unceasingly developed morality, has made it more and more regnant? Can I interpret it otherwise than in terms of reason and goodness?

6. If the value of religion has been found more and more in its ethical content, will not it be merged in morality and the

specifically religious cease to exist? In answer it may be said that religion has roots in man's nature independent of morality. Man will not cease to be religious and metaphysical. "We still recognize our absolute dependence upon the power that brought us hither and will conduct us hence."

The Contents of Religious Consciousness. By J. H. LEUBA.

Causes of Scepticism. By E. H. SNEATH.

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THE CRITERION OF SENSATION.

BY PROFESSOR GEORGE STUART FULLERTON,

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In a paper read a few years ago[1] before this Association, I tried to show that psychology, as a natural science, rests upon the same general assumptions as the other natural sciences, and that, however accurately it may describe the contents of consciousness, it nevertheless views the mind and the world from a standpoint which does not differ greatly from that which characterizes the thought of the unscientific. I pointed out, however, that the very clearness and thoroughness with which the psychologist deals with his subject throws into relief what seems a fatal inconsistency in the very foundations of the doctrine regarding the knowing mind and its relation to the world held by the plain man and the psychologist in common, while the vagueness of the thought and speech of common life serves to conceal, to some degree, this same inconsistency. I allude, of course, to the doctrine of representative perception, which distinguishes between things and our ideas of things, regards the former as causes[2] of the latter, and teaches that a mind can know directly only so much of things as is included in the representatives of them which are immediately present to it, and can reach only by inference whatever may be supposed to lie beyond.

It was shown in the paper to which I refer, that the inconsistency in question is imbedded in the very nature of the doctrine. The mind is completely cut off from external things, which it is supposed to know exclusively through their proxies; and yet the whole scheme of representative ideas and things represented by them is built up by tacitly assuming that it is pos-

[1] Printed in the PSYCHOLOGICAL REVIEW, March, 1894.

[2] I do not insist upon the word 'cause'; it does not concern this paper to discuss the point.

sible to know certain external things at first hand, to perceive the true relations of ideas and things, to compare copies with originals, and to trace the construction of a successful copy in detail, thus following step by step the development of the idea of an external world in this mind or in that. A flatter contradiction than this it is impossible to conceive. It is ideally flat. And yet it was maintained that this inconsistency in the psychological way of viewing the mind and the world need not diminish the value of the work done by the psychologist, that he may with profit continue to study mental phenomena in his own way, and that he may leave the whole problem of the elimination of this contradiction to someone else. This someone else is the epistemologist, whose business it is to work beneath the plane of the common understanding, and to analyze and explain concepts which those who remain upon that plane may very well use without analyzing them.

This view of the work of the psychologist, and of its relation to that of the epistemologist, I wish now to illustrate by a brief discussion of the question: What is a sensation? I shall try to show that any answer to this question, that can be given by psychology as natural science, cannot be regarded as ultimate except for convenience of discussion within a given field of investigation; that the justification of the answer given must be sought in the domain cultivated by the epistemologist; and that the contradiction which has been shown to exist in the psychological standpoint is innocuous simply because it is a contradiction only to the psychologist, and not to one who carefully analyzes his conceptions and comprehends clearly the use which he actually makes of them. What I have to say is neither very recondite nor very original, but it seems to me worth while to say it, nevertheless.

The importance of the part played by sensations in the doctrines of the psychologist can scarcely be overestimated. From them he sets out, and to them he finally returns. Upon them he bases, directly or indirectly, everything else that enters into his calculations—percepts, memories, the most abstruse and abstract chains of reasoning, the loftiest and least material of ideals. Sensations ultimately furnish the material for the construction of

his whole edifice, and at the same time they give to it whatever of stability and reality it may possess. For it is in sensation, and in sensation alone, as it is assumed, that the mind has direct contact of any sort with an external real world, receives messages from it, and can know that it is reacting upon it and playing a real part in a real system of things. When we leave sensations, and deal with those ghosts of them that Hume called 'ideas,' we leap into the air, and our leap is worth making only because we can again return to sensations, and once more plant our feet upon the solid ground. If such be the importance of sensations to our psychology, it is surely desirable to know with some clearness what a sensation is, and to be able to furnish some criterion by which it can be recognized to be such. What criterion shall we assume?

There are those who are inclined openly to justify the tacit assumption made by the psychologist that he actually perceives immediately external things, and who deny his formal statement that such are known only through their representatives in consciousness. They hold, in effect, that we immediately know both things and the mental complexes which stand for things, and that, being conscious of both, we are in a position to compare them with each other, and to pick out those ideas which really correspond to things from those that do not. This appears to furnish us with a very short and easy method of distinguishing sensations from other mental experiences.

But this denial of the doctrine of representative perception throws into the direst confusion the whole account given by the psychologist of the sense-organs and their functions, and makes incomprehensible his doctrine of the gradual emergence in an individual consciousness of the idea of an external world of things. If our knowledge of things is in any sense independent of the percepts which have been built up out of the messages conveyed to the brain along the sensory nerves—if the things themselves can be directly known by the mind 'alongside' of the percepts, there seems no reason at all why our knowledge of things should be a function of our percepts, and the exactitude of that knowledge in any way conditioned by the number and character of the percepts which we have, at any given time,

succeeded in building up. Why should not an infant, at the very outset of its career, before it has any percepts at all, nevertheless know real things just as they are and in all their complexity? It is by no means evident that percepts, sensations, brain, nerves, and peripheral sense-organs are at all essential to that immediate knowledge of things which is independent of their representatives in consciousness. The progressive development of a consciousness of things is an explicable fact upon the basis of the psychological principles generally accepted. It is a wholly bewildering fact upon the hypothesis that things are immediately present to the knowing mind *beside* the percepts which are supposed to correspond to them. On this supposition how is it possible for any mind, at any stage of its existence, to be ignorant either of the existence of objects or of their precise character? Are we to explain such ignorance by assuming that these objects, which are thus immediately present to the mind, keep changing to keep pace with the development of their mental representatives? This seems absurd. Yet if an object be present from the first as it really is, I cannot see that it matters at all what degree of imperfection there may be in those transcripts of external things which we call percepts. The mind may conceivably know things just as they are, however inadequate the images of things which it has succeeded in building up, or, for that matter, whether it has built up a store of such images, or has not yet begun to do so.

Well may the psychologist beg to be delivered from the friendly offices of those who would defend him, if his defense is to be undertaken in this outrageous fashion! His tacit assumption that he directly perceives the world of material things and knows immediately that he is dealing with such when he is observing or experimenting upon a human body, was, after all, only a tacit assumption; it was smuggled in quietly and was never intended to be applied recklessly, *semper ubique et ab omnibus*. He may assume that *he* directly perceives the body of his subject and the visible object toward which its eyes are turned. But he stoutly denies that *that subject* sees this external object, except in the sense that, as the result of a certain physical action upon his body and the subsequent nervous re-

action, there arises in his mind an image or representative of it. The image is allowed the subject; the thing itself is denied him. It is strenuously maintained that whatever may be the properties of this outer thing, they are as nothing to the subject unless they are reproduced in that image. The whole domain of the psychologist appears to be devastated if this, his fundamental assumption, be denied.

It seems almost unnecessary to discuss at length this preposterous doctrine of an immediate knowledge of things external independent of the percepts which represent things. Yet the doctrine has been seriously advanced again and again, and in a variety of forms. Sometimes it is advanced in so vague and insidious a form that its full significance is not easily grasped, nor is its absurdity clearly apparent. We are asked to believe, for example, not that the mind knows *things* independently of its percepts, things with the definite properties that make them what they are, but that it knows *reality*. Why it should thus know the something called 'reality' rather than the definite forms of real being which things seem to us to be is never made apparent. If we can know anything except *in* or *through* (*i. e.*, by an inference from) our mental representatives of things, there seems no reason why we should not thus know everything, and our whole circle of percepts becomes a mere epi-phenomenon, a useless thing, valueless baggage with which the psychologist bustles about to no purpose. Well may the psychologist stand aghast at the logical implications of this doctrine brought forward in his defense.

It seems, therefore, both wise and charitable to allow the psychologist to persist in his denial that we know directly anything beyond the mental complexes which he endeavors to analyze and classify; and, in that case, we must not expect him to distinguish between sensations and other mental experiences by the simple expedient of picking out those experiences that can be immediately perceived to correspond to external things or to aspects of such things. To what criterion shall he turn?

There is, of course, the greater degree of vividness which serves to mark out roughly our sensations, as a class, from what Hume calls our 'ideas.' But it is impossible to overlook the

fact that this difference in vividness is by no means an infallible criterion, and that, consequently, some other must be found if we are to feel quite safe in relegating a given experience to the one class or to the other. A psychologist will note the fact that our sensations are usually more vivid than their copies in memory and imagination, and he may point out the truth that this fact is of much importance in practical life; but no psychologist would dare to make the vividness of a given experience the sole basis of its claim to being a sensation. Hume's 'ideas' may, in certain cases, be very vivid and insistent; sensations may be extremely dim and shadowy. The inarticulate murmurs with which the body protests against impure air or ill-fitting clothing we recognize as sensations, while we banish to a different class the most impressive of hallucinations and the most distinct of figures seen in a dream.

We must, then, discover some other final court of appeal, if the claims of sensations and 'ideas' are to be determined with anything like an approach to justice. Such a court the psychologist tries to furnish us in distinguishing between mental experiences which are to be regarded as the result of a 'peripheral' stimulation, that is, those which come into being when the outworks, so to speak, of our nervous system are thrown into a state of activity; and mental experiences which correspond to a relatively independent activity of the 'central' nervous system, those, in other words, which represent brain action which is not a direct response to a message conducted along a sensory nerve. The distinction appears, at first glance, to be a convenient one. Perhaps it will really be a very convenient one when we possess a more accurate knowledge than we now do of what takes place in the peripheral nervous system and in the central under such and such circumstances.

But what is meant by this appeal to the peripheral nervous system? If the psychologist assumes that he knows an external body with its nervous system independently of the whole circle of his sensations and 'ideas,' he involves himself, as we have seen, in no end of trouble. To save him from this, we must do him the credit of believing that, whatever be his modes of expression, and whatever the vagueness of his thought, he does

not really mean this, or at least does not depend upon such an assumption, but adheres to his doctrine of representative perception. When, therefore, he tacitly assumes that he knows his own body and certain other bodies immediately, we must understand him as having reference to certain complexes of sensations and 'ideas' which he recognizes under those names. At once the question arises: How can he prove these experiences, which are to furnish the touchstone for judging other experiences, to be of the class called sensational? to be composed, in part, at least, of sensations? If he simply assumes that they are such, as he apparently does, and then uses them as a test of other experiences, is he not, in his whole investigation, simply guessing at half the distance to the sun, and then multiplying by two, in order to discover how far away the sun really is? And what becomes of the doctrine of representative perception itself, if it is quite impossible to prove satisfactorily that certain complexes in our consciousness really represent external things, and if this has to be arbitrarily assumed at the outset. The psychologist appears to be forced to choose from a pair of dismal alternatives: either the mind *is not* shut up to the circle of its sensations and 'ideas,' with what may be constructed out of these, and then the house of the psychologist rests upon sand that will not bear the test even of dry weather; or the mind *is* shut up to the circle of its sensations and 'ideas,' and then it is only by making a quite arbitrary assumption that the psychologist can gain a criterion which will enable him to distinguish between sensations and their copies, and to affirm that now his experiences represent real things and now they do not.

The position of the psychologist is not, however, so bad as it looks when set forth in this way. The assumption that external things can be known independently of percepts is, to be sure, really fatal to psychological doctrine, and can be seen to be so by anyone who will patiently develop the consequences of the assumption. But, for the other alternative, does it really follow that, if we accept seriously the psychological doctrine that a man is ultimately shut up to the circle of his ideas (here taking the word in its broad sense), it is impossible for him to arrive at a knowledge of real things, and to distinguish between

sensations and mere copies of such? Is the psychologist's assumption one for which no reason can be given?

I have maintained that the psychologist remains upon the plane of the common understanding, and assumes that he has a right to assume certain things even if just what is implied in these assumptions is not fully understood. This does not necessarily mean that such assumptions are made arbitrarily. It only means that he does not, as a psychologist, feel called upon to prove his right to make them. When we set ourselves to work to examine them systematically, we may find that he was perfectly justified in making them, and that he did so because he found in his experience abundant reasons for so doing; but we may also find that he was but dimly conscious of these reasons, and that he is in some danger of misunderstanding the actual significance of the assumptions that he has made. We have seen that the psychologist assumes that he is assured of the reality of an external world of things in which human bodies play a part, and that he appeals to this in his endeavor to furnish a criterion of sensation. We have seen, further, that if we try to justify this assumption in one way, we simply undermine the foundations of psychological doctrine. And yet there must be some way in which it can be justified; psychology does rest upon this assumption, it is a science, and it does prosper. It cannot be either wholly baseless or hopelessly inconsistent. The psychologist may not feel called upon to look into all this, but, in the interest of clear thinking and accurate knowledge, it is surely desirable that some one should do so.

Now what sort of evidence is it that has led the psychologist to assume that he has a touch-stone by which he can distinguish sensations? He has had a life-long experience of the fact that his experiences fall into two classes, marked by differences in the way in which the experiences behave, by differences in the order of their appearances and disappearances. The child is quite capable of recognizing that the lion which appears when the blankets have been tucked in, and he has been abandoned to the phantom terrors of a solitary crib, is not exactly like the lion that exhibits himself by day in a cage, and which can be seen only by paying admission. The behavior of this lion is

too inconsequent. He is real enough to inspire fear, but he is, nevertheless, not exactly real. The presence of the light is enough to exorcise him. And even the man who has no settled opinions touching the existence and nature of ghosts is apt to think that the ghost that can be photographed is more real a ghost than the one which can, at best, only make itself apparent to the terrified rustic at dead of night. We have all our lives been judging our experiences, and arranging them as a result of that instinctive judgment. What we see we try to touch, and what we touch we perhaps try to taste and smell. When such direct tests are out of the question, we can apply, as we learned long ago, certain indirect tests that may be regarded as their equivalent. But tests of some sort we must and do apply, and we have been applying them with a good deal of success, and with, hence, an unshaken confidence in their validity, during all the years over which memory extends. No one approaches mature life without finding himself in a world of things pretty well known, and without settled habits of testing things to find out whether they are *real*, *i. e.*, whether they belong to that orderly class of experiences which have fallen into a connected system, or whether they defy such an arrangement and must be relegated to a class of a different kind.

Hence it does not occur to the plain man to offer proof that his body is real and that it is rather intimately connected with the knowledge of other things. He knows that it is real. It is an old friend, whose stability of character cannot be brought into doubt by the officious questions of the irresponsible sceptic. And as to the part it plays in the knowledge of other things, a man has not arrived at years of discretion who does not know that when he shuts his eyes things are, for him, snuffed out; and that when he sticks his fingers into his ears, the sounds he has been hearing give place to silence. All these things are experiences which have been found to fall into a fixed and orderly system. That system is for him the system of real things. His body and certain other bodies evidently belong to it, their reality does not have to be proved, and he only busies himself to prove the reality of those things that are still in doubt. He may raise the question whether what seems to be a human body

faintly and vaguely perceived is really a human body or a delusive appearance, but he does not raise the question of the reality of the eyes with which he is looking.

Thus the plain man finds himself in a real world of things at the outset of any given investigation. He endeavors to extend his knowledge, but he sees no necessity of trying to prove that, as far as it goes, it really is knowledge. Before he recognized with even an approach to clearness that his experience presents him with 'ideas,' which should be carefully distinguished from things, and which sometimes pass themselves off for things in a conscienceless way, he felt himself to be in a world of things, real, insistent, unmistakable, and, at least to some extent, law-abiding. It seems to him simply absurd to say that perhaps he is not really conscious of a world of things at all, but is wholly shut up to a world of mere copies and appearances.

And the psychologist is evidently in the same case. He emphasizes, it is true, the distinction between things and their mental representatives, and boldly puts forward the doctrine of representative perception; but, as we have seen, he always tacitly assumes, nevertheless, that he is in a world of real things, is conscious of such things, and can use this knowledge as a basis of his reasonings. His criterion of sensation, the reference to the peripheral nervous system, is merely a refinement of the unconsciously assumed criterion of the plain man, and it is, moreover, a refinement of which, in the present state of our knowledge, we can make but a limited use. What psychologist would dare, on a bare inspection of some part of the peripheral nervous system, to declare that a given individual is experiencing a sensation? He moves a light before his subject's eyes; there is a certain reaction; he infers the presence of a sensation; he leads the subject to believe that a faint light will be presented; he does not present the light; there is the same reaction; he infers that the subject has mistaken an 'idea' for a sensation. In all this he reasons just as the plain man reasons, and if he afterwards talks of peripheral and central nervous activities, it is not because an immediate perception of these has furnished the basis for the conclusions at which he has arrived. Even if—a consummation

devoutly to be hoped for—we some day succeed in attaining to such an acquaintance with the nervous system as will make possible an accurate knowledge of its reactions, in part, at least, at first hand, we shall still find the psychologist doing substantially what is done by the plain man, when he tries to discover whether a given experience is sensational or imaginary; we shall find him trying to fix its relation to a certain mass of experiences already recognized as real. We shall find him referring it to a world of things, a world which, as he cannot doubt, is given in his experience. The fact that he can proceed rather mechanically, by the application, as it were, of a convenient formula, merely signifies that he can do his work without being compelled to have recourse to that which gives his formula its significance.

Here it will be objected that I seem to be slurring over the distinction between things and their mental representatives, a distinction which is nevertheless recognized both by the plain man and the psychologist. If I describe the process by which a man arrives at a consciousness of a world of things as nothing more than the recognition of the fact that certain of the elements in consciousness fall into a certain orderly system; if I conclude that any element must be assigned a place in the real world, or refused such a place, not by virtue of some inherent peculiarity of its own, but by virtue of the peculiar nature of its relation to our other experiences; if, in short, I refuse to admit a world of things quite beyond and numerically distinct from the whole circle of our sensations and 'ideas,' but somehow include in consciousness things 'outer' as well as things 'inner,' then am I not contradicting both the plain man and the psychologist, who certainly appear to believe that they find themselves in a real world of things quite distinct and different from consciousness-contents of any sort? When these men speak of the body, do they not mean something quite distinct from all experiences of the body?

To this I must answer, it is rather a nice question to determine what a man means when he does not know what he means himself. The plain man evidently has the vaguest possible notion of what he means by external things, and of the significance of the

distinction between such things and their mental representatives. And although the psychologist brings this distinction into sharper relief, he does not even attempt to answer the questions to which it so naturally gives rise. Our psychologies do not begin with definitions of reality, or investigations into the possibility of representing real things by means of mental complexes. They assume that we all have that somewhat indefinite knowledge of things which belongs to common life, and they begin building upon that basis. Hence the epistemologist need not feel that he is opposing the plain man and the psychologist, when he refuses to accept certain of their statements regarding their position. He may claim that in making such statements, these men have wandered away from their own field; that they have become, for the moment, epistemologists. He knows well that those who are epistemologists only for the moment and as the fit seizes them, who have given no serious and continuous thought to the problems with which epistemology deals, may easily misconceive the significance of the concepts which they undertake to analyze.

I do not wish to terrify this Association by leading it to suppose even for a moment that I intend to expound in detail in this paper just what the epistemologist means by the external world and its representation in an individual consciousness. It would require a lengthy discussion to do justice to this theme. But I hope enough has been said to make it seem not altogether unreasonable for the epistemologist to take at least the following positions:

(1) The psychologist is not wrong in assuming that he has an immediate knowledge of the real world of things, and he is justified in working upon this basis.

(2) Nor is he wrong in distinguishing between things and the complexes in consciousness which stand for them—between things and, using the word in a broad sense, ideas of things. The doctrine of representative perception has not been framed arbitrarily, as its usefulness to the psychologist abundantly proves.

(3) It will not do to eliminate the contradiction in the psychologist's assumption that he does and at the same time does

not immediately perceive real things, by denying the doctrine of representative perception. This can only result in hopeless confusion.

(4) The contradiction can only be eliminated by recognizing the fact that percept and thing perceived are not two things numerically distinct and outside of each other, but are two ways of regarding the same complex in consciousness. The distinction between complexes in consciousness and the real things for which they stand is, hence, a distinction within consciousness; and it is not pure dogmatism, such a dogmatism as the metaphysician is sometimes accused of, to speak of the relations between the inner and the outer worlds.

(5) But the elimination of the contradiction in the psychological standpoint is the duty of the epistemologist, not that of the psychologist. For the latter, the distinction between things and percepts is unanalyzable. To see its true significance one must leave the plane of the common understanding, which means that one must leave the plane of natural science. And since the psychologist, in offering us the criterion of sensation which he does offer us, makes use of this distinction without attempting to analyze it, his criterion is, as has been maintained, ultimate only to those who remain within a certain field. He bases himself upon experience, it is true; if he did not, his work would be valueless. But he does not furnish us with an analysis of the experience upon which he takes his stand. It is not necessary to his purpose that he should do so.

DISCUSSION AND REPORTS.

PHYSICAL PSYCHOLOGY.[1]

A few years ago I heard a paper on an old subject, 'The New Psychology,' that was amusing for several reasons, but for nothing more so than the classification with which it opened. Like Gaul of long ago, psychology was divided into three parts: rational psychology, experimental psychology and biblical psychology. Now, however, outdoing that modern Cæsar, I would—and possibly not less to your amusement—add a fourth province to the list, namely, physical psychology. Physical psychology is concerned with the substitutes or indirections for mind that appear in all the so-called physical sciences, in chemistry, physics and mathematics. It is, of course, quite unconscious of itself as psychology until it has been exposed, being at least one degree more remote or indirect and objective than physiological psychology, but its unconsciousness and remoteness can not make it less interesting or less serviceable to a sober student of mind.

Of the abstractions or indirections for mind in the physical sciences I made mention in a paper before the American Psychological Association in 1897.[2] The general principle upon which I relied at that time was that psychological or epistemological sciences and physical sciences, which assume the dualism of mind and matter and are accordingly separated from each other, must reproduce, as if by projection on their own planes or within their own peculiar spheres, the same dualism,[3] or more generally and with a possible suspicion of Hegelianism, the terms of any antithesis, opposites of any kind, do and must individually reproduce their opposition, and this the more if the opposition is absolute. Indeed, since opposition means difference, things that are different in any degree must one and all *individually* reproduce the whole system of differences to which they belong.

[1] This paper was read by title before the American Psychological Association at the New Haven meeting in December, 1899.

[2] See *Philosophical Review*, July, 1898: 'Epistemology and Physical Science—A Fatal Parallelism.'

[3] For example, the atoms and conserved matter of chemistry, the things that move and motionless transmitting medium of physics, the finite quantities and infinity of mathematics, and the sensations and thought or apperception of psychology.

Put in this way, the principle is seen not merely as an exposure of dualism, but also as an account of all differentiation. Any individual, for example, is made a whole-containing part, a microcosm, and its life-history, not literally perhaps, but still in a sense also thoroughly genuine, recapitulates its entire development. But this more comprehensive application of the principle of difference or opposition is not a part of my present purpose, although the mention of it has seemed to me to be serviceable. As already implied, I wish to consider only the special case of dualism and particularly the transformation or exposure of physical science that the principle effects.

That opposites do reproduce each other or take sides with each other, being each invisibly what the other is visibly or intensively what the other is extensively, may require no more than statement, but still I would suggest as illustrations such opposites as good and evil, religion and science, organic and inorganic, conservatism and radicalism, spiritual will and physical force, the one and the many, idealism and materialism. In each of these cases either of the opposed factors is hidden or implicit in the others. Virtue as only the negation of vice is always a party to vice. Unity, that is an abstraction from plurality, is empty and so, although perhaps extensively one, is many intensively, and plurality, that is an abstraction from unity is still intensively one, being plural only by reason of what can be but indifferent differences. In the early Greek philosophy, because Being and not-Being, the One and the Many, individually reproduced their opposition, Heraclitus was able to identify them in Becoming. And the regularity with which in ancient or in modern philosophy materialism and idealism have passed into each other and even through each other is well known.

But physical psychology can justify its claim to recognition in other ways. An abstract principle that in spite of profuse illustration the timid among thinkers may find offensive for being paradoxical, is not its only witness. The early Greek philosophy is now very generally recognized to have been busied with physical abstractions for mind. Thus Anaximander's Boundless, the Eleatics' Being, Heraclitus' Fire or Becoming, the Number of the Pythagoreans, Anaxagoras' Nous and Democritus' Vacuum were all concepts of a physical psychology. No mere accident made the Vacuum of Democritus and the Concept of Socrates contemporary developments in the history of philosophy. And then, as if by what a biologist might call the present's recapitulation of the early Greek philosophy, our modern physical science is busied also with mind under physical disguises. Conservation as a

doctrine of constant amount, infinity as a quantity, physical plenitude as meaning that what is between things is as real as the things themselves and motion as defined by Newton's laws, or in any other way that would judge it extensively, only are each and all of them abstractions for mind. This is to say that in them all we can discover, we can unmask the essential character of mind; and no wonder, for are they not all openly treacherous to the categories of matter or of physical reality generally, being in meaning and use, when not also in form, veritable negations of matter within its own camp? The books of the day[1] that more or less clearly recognize these treacherous negations are actually rivaling the historical novels in number.

No scientist at the present time would hold that conservation is of a finite quantity, whether of matter or mass or energy or anything else you like, and yet the only quantities are finite quantities. Infinity, to which conservation is applied, is not a quantity, being an open denial of the very limitations that make quantity. Conservation, then, instead of referring to mere quantitative constancy, may even be a principle of quantitative change. What it does assert is just such unity and indivisibility in the world as makes the discovery of law possible, for without conservation mind, which is also one and indivisible, could not find itself reflected in the physical world. Indeed it seems no extravagance to me to say that conservation, however physical in its standpoint, is only an indirection for self-consciousness or at least for what Kant has called the unity of apperception. By its assertion of the constancy of a quantity that is infinite it unwittingly makes the physical world participate in the nature and specifically in the unity of mind.

In regard to infinity some may hold that it is a quantity, but, if it is, then all quantity must be more than mere extension, infinity being only a witness to the intension of quantity in general. Thus, on the assumption of infinite divisibility space and time have parts that are coextensive and contemporary respectively, and this is but to make them virtually indivisible and to give them a unity of intension, which is, again, the very unity that we commonly ascribe to mind. Mind or thought might even be defined as that in which the parts of space are made coextensive—or mutually inclusive—and those of time contemporary. Such coextension or contemporaneity of parts is, of course, paradoxical, but the conception of it is as legitimate as infinity and the paradox only shows how thought can transcend its forms.[2] More-

[1] Ward's 'Naturalism and Agnosticism' is a noteworthy example.

[2] Mathematics has many illustrations of thought transcending its forms. Consider, for example, its use of surds, irrationals, imaginary quantities, incommensurables, and the like.

over, the familiar law of relativity, as a law of mental states, can mean no less than that the parts of experience in space or time are mutually inclusive, since it makes all experience one and indivisible.

But, thirdly, of a kind with conservation and infinity there is the conception of plenum. The plenum, relatively to what is in it, is a vacuum. The ultimate ether with all its ideal properties of impenetrability, rigidity and the rest is not only between all things, but also must permeate all things, and so being necessarily different in kind it can really be only an indirect representation of another sort of continuity than that of mere physical contiguity. An organism, for example, whose parts in a very genuine sense are actually coextensive and whose temporal stages, if there be any truth in recapitulation, are contemporary, is continuous in another way than that of contiguity, for mere contiguity is purely statical. This, too, is more than an illustration here, for at the present time both chemistry and physics seem to be preparing for the admission of organic evolution to their sacred precincts; in short, to be near to an identification of material reality with the nature of organism. The continuity of organism, however, is quite consistent with the intensive unity of mind.

And, finally, intension is as much a truth of motion as extension. Newton really corrects the false standpoint of his laws by the provisos with which they close. Thus a body in motion continues to move only if not resisted, and a body at rest remains at rest only if not disturbed. Moreover, one can accept these laws, and many do accept them, and at the same time believe neither in the absolute rest nor in the absolute motion of anything. But were motion only extensive, such absolutes would be necessary; so that in denying them or in recognizing as universal the resistance of motion and the unrest of inertia the physicist is really admitting intension to his idea of motion. Motion has intension, if it is in an infinitely divisible space and time, whose parts, to repeat once more, are coextensive and contemporary, and this is to say if it is not the right and property of separate individuals, but the inner activity of an indivisible whole, being identical perhaps with organic interaction. So early a thinker as Zeno, the subtle logician of the Eleatics, was at best very near to recognizing the intension of motion when through the use of the idea of infinity he found rest to be one of the truths of motion. Neither rest nor motion, he seems to me to say, belongs to any individual thing peculiarly; the two are not mutually exclusive states; rather they are truths about each other, rest being the intension of motion. Zeno's paradoxes were only his way of saying, to quote from above, that

motion is but the inner activity of an indivisible whole and modern science, using the infinitesimal and through this able to describe motion in the laws or formulæ of a pure mechanics, is justifying him. The rest of motion makes a science of motion possible. To perception, then, motion may be only extensive, but to conception, to scientific formulation it is also intensive. The rest of motion is the unity of mind that all lawfulness reflects.

Of course, I shall be accused of trying to establish physical psychology upon the mystical ground of a vision of the invisible, but, to say the least, the accusation, if it come from physical scientists, is a most fatal boomerang, for what science, however objective, does not rely on a vision of the invisible? The intension or rest of motion, with which I would identify the unity of mind, may be quite beyond the possibilities of sensuous imagery, but what of that? Science has yet to dismiss from the respectable company of its conceptions the conception of the infinite, upon which this invisible rest of motion is established, and of all things infinity certainly outruns the imagination. Moreover, in this case as in so many cases, what we cannot find when we look for it, what we cannot consciously imagine or analytically construct is in fact actually and immediately present to us. Every physical quality, present to consciousness as sensation, is nothing more nor less than the rest or intension of motion. And then, furthermore, even an objective physical science, which would see everything in extension, is not wholly helpless, for it has its ways of escaping its limitations. Thus, to give a simple example, in rotation, so important to modern science, motion is shown to be intensive, for (1) it appears as responsible to its starting-point as to its goal, (2) it expresses a difference that is only in degree, (3) it is consistent with the principle of conservation, (4) it is rest as well as motion, and (5) it is a resultant of opposites. As only extensive, motion could be but sheer escape or sheer change of place, having no persisting responsibility to its starting point, and it would imply difference in kind and deny unity or conservation and be only so much lawless or brute momentum.

So, to summarize and conclude, the claims of physical psychology to recognition lie, first and most generally, in the fact that opposites must individually reproduce their opposition, a fact which forces into physical science certain abstractions or disguises for mind, and, secondly and specifically, in the witness that such abstractions as conservation and infinity and plenitude and motion bear to the presence of intension, of the unity and indivisibility of mind, in the physical

world. Physical science has long been fond of boasting a freedom from paradox and contradiction, resenting the subtle dialectics of philosophical inquiries, but if the thinking here be correct, the boast is a most hollow one. The hidden paradoxes of physics turn physics inside out, they turn it into psychology and philosophy.

Of course, physical psychology is something of a mongrel, but let me say that crossing the sciences is becoming a sign of true sportsmanship among scientists.

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PSYCHOLOGICAL LITERATURE.

Aberglaube und Zauberei von den ältesten Zeiten an bis in die Gegenwart. Von Dr. Alfred Lehmann, Direktor des psychophysischen Laboratorium an der Universität Kopenhagen. Deutsche autorisierte Ausgabe von Dr. Petersen. Stuttgart, Ferdinand Enke. 1898. Pp. viii+556.

For the student of culture and of the mental processes upon which culture depends, the story of human error and of misdirected effort is as instructive as the history of truth and of progressive achievement; for the tendencies which develop forms of belief are quite as characteristic psychologically, whether these beliefs survive the tests of a more advanced science or are weighed in the scales of a critical judgment and found wanting. Parallel with the evolution of scientific knowledge and of conceptions and principles, is an evolution of logic. A form of evidence or of argument which seemed formidable to the scientist of only a few generations ago may to-day seem trivial and irrelevant; and this partly because of the increased knowledge and new conceptions which explain the phenomenon in question in a different and more thorough manner, but also partly because our perspective of logical values has been notably modified by the inheritance of the ages. This contrast becomes all the more striking when we attempt to measure the thought of a remote age and of a different form of civilization by the standards of our own period. The scientific conceptions of Chaldean and Egyptian, of Greek and Roman, or of the mediæval scholar are so at variance both by what they lack and by what they present—by their standards of credibility and by their entire logical attitude—with those which we inhale in our mental atmosphere, as to require a considerable effort and, indeed, a special imaginative gift to bring about a sympathetic and appreciative comprehension of this ancient lore. The continuity of development, however, brings it about that beliefs once prevalent are but gradually superseded or discarded; they may disappear among the more cultivated, but culture, even to-day, affects only a moderate portion of the community. The conservatism of tradition is more potent than the advance of new truth. And, moreover, from time to time, the notions of a bygone age or of a

more primitive culture reappear, possibly in more modern garb but still recognizably the same, to find adherents among intellectual malcontents, who regard the dominant attitude on this or that topic unsatisfactory or false, and are desirous of replacing the slow methods of science by the more brilliant achievements of magic or inspiration. The existence in our midst of adherents of notions and systems which were dominant in a bygone age, the cropping out among us of movements the propagation of which proceeds by appeal to the same impulses and tendencies which pervaded the mystic lore of the past, make it more readily possible to understand the attitude of the original adherents of these beliefs.

Dr. Lehmann's work is a notable contribution to the psychology of human error; it is a valuable aid to the student who wishes to form an acquaintance with the pseudo-science and the primitive science of the past, and to appreciate the relations of antiquated lore to its modern survivals and revivals. It forms a thesaurus of such information and it reviews this information in a psychological light, which is at once clear and helpful, a light which is modern and critical and direct. Pseudo-science, the occult, human error, superstition—however we may term it—comprises the products of certain mental stages and tendencies; and these formulated products contribute the subject material of the volume. Dr. Lehmann calls it superstition and includes therewith magic, which he aptly characterizes as superstition reduced to practice. But these superstitions, though they may be but incidental in a later civilization, reflect the essential habits of thought of a less developed period. They constitute the replies to serious questionings and grave speculation. It may be 'Aberglaube' to us, but it is 'glaube' to its adherents; and therein lies its psychological as well as its historical significance. The field of inquiry thus suggested has not been frequently covered in a comprehensive and critical spirit; and of specifically psychological studies dominated by an appreciation of modern psychological conceptions there are almost none. For this reason the present volume is likely to occupy a prominent place as a guide and *vade mecum* for the student of this interesting field. It, therefore, seems proper to indicate the scope and general contents of the volume.

While the historical portion is considered as introductory to the account of modern 'superstitions' and the interpretation of the entire range of phenomena, it occupies a very considerable portion of the volume. After a brief review of magical practices amongst primitive peoples, the magic and superstition of Chaldea, of Greece and Rome,

of the Jews and the early Christians, of Norse and Finns are presented in interesting outline. This lore is concerned, as it continues to be concerned almost up to our own day, with auguries and portents, with the treatment of disease, mainly by driving out demons and by exorcism, with influencing and predicting the future by astrology or clairvoyance or fortune-telling, by oracles and divination, and with the attempt to combine these various practices into a more or less consistent and comprehensive system—a system frequently involving conceptions of cosmogony and religion. The elaborate development of Chaldean magic, the indirect dependence of later occult lore upon it, the success of modern exploration and research in bringing to light these early records of human speculation, contribute to give the magic of Chaldea a fundamental historical position. In no other way can one be so impressed with the generic similarity of the psychological processes and ambitions that pervade this long record of human endeavor to pierce the mystery of existence and to steer one's craft by an occult compass, than by viewing the history of superstition and magic against the background furnished by the lore of Chaldea. A special interest also obtains to the cult of Norseman and Finn, because of the originality and relative independence of this development. The author's personal and local interest in these traditions is properly reflected in the more detailed delineation allotted to them.

The mediæval development of Christianity brought essentially new contributions to magical practices and to magical systems. The epidemics of witchcraft represent acute culminations of dominant beliefs; but these outbreaks are closely related to elaborate demonologies and various superstitious conceptions of God and nature. Dr. Lehmann devotes a special section to the occult 'sciences' which flourished so luxuriantly in the superstitious atmosphere of mediævalism. Astrology and alchemy, the Kabbalah and other forms of symbolic interpretation, the doctrine of sympathies and antipathies, the treatment of disease by methods founded on fanciful analogies, the search for the philosopher's stone and the elixir of life and the transmutation of metals, the speculations of an Agrippa or a Paracelsus—are replete with illustrations of the results of learned error, in the absence of a scientific logic and of the saving conceptions which such logic engenders. They furnish the text alike for logical and for psychological sermons and parables.

We must certainly modify the current associations of 'superstition and magic,' before we can include under this head modern spiritualism and theosophy and occultism and demonstrations of the

fourth dimension—at all events before we can do so without provoking a general alarm in many vigilant camps. In reading the volume one finds the transition easy and natural from mediæval demonology to modern spiritualism. In the accounts of critical investigations of 'spiritualistic' phenomena one encounters a logical attitude quite foreign to that of preceding ages; and the phenomena presented are unmistakably clothed in modern fashion. But the atmosphere of the séance chamber has a sympathetic suggestion of ancient magic, and the logical status of speculations about astral bodies and reincarnations is not altered when they are talked about in the language of the nineteenth century.

The last section of the volume represents the psychological portion. The dominant tone is explanatory. It considers the natural tendencies of mind favorable to the formation of 'superstitious' conceptions; by the aid of recent advances in the domain of psychology, it analyses the phenomena exhibited in the past. An account of normal errors of observation—as exemplified in the effects of interest and attention, of the success of a prestidigitateur or in the classic observations of Mr. Davey—furnish an illuminating though not a complete psychology of deception and prejudice and the propagation of contagion. The study of unconscious and involuntary movements, of dreams and somnambulism, of the varied phenomena of hypnotic suggestion, gives to accounts of possession and trance and religious ecstacy an intelligible interpretation. The scientific study of the action of drugs and particularly of the peculiar mental concomitants of hysterical and epileptic conditions, enables the modern student to detect in the distorted descriptions of fanatical witnesses a consistent illustration of mental disease. The most truly objective phenomena, the residue of fact, which under the potent superstitious conceptions swelled to an enormous body of belief—composite of tradition and ignorance, of an inflamed imagination and an ascetic theology, increasing by the momentum of personal feeling and popular unreason—thus become somewhat stripped of their mystery. Much remains to be accounted for, and the importance of the mental atmosphere in which alone superstition thrives is not to be lost sight of; but a review of these phenomena in the light of modern psychology enables one to see the character of the forest and prevents one's bewilderment by a mere aggregation of trees.

The intrinsic merits of the work are manifold, especially in contrast to other contributions to this field. The author's conception of his task is admirable, his industry untiring, his estimation of relative

values sound. The execution fails in more than one respect; there is little continuity of method in the historical portions; the reader feels that his attention is drawn hither and thither not for adequate reason, but rather by neglect of a finished composition. Useful as may be a reference handbook to the occult, a more organic treatise would be still more valuable. How discerningly the historical material has been collected must be left for the historical specialist to decide. Important omissions in the accounts of modern 'occult' movements will be noticed, particularly by the American reader. In the account of spiritualism the report of the Seybert Commission—which from the author's point of view is perhaps the most important contribution to the topic—is not mentioned. The account of unconscious movements would have been more forcible if it had been based on a more extensive knowledge of the literature. These defects of execution and detail should be stated, but they should not be allowed to interfere with the general favorable estimate of Dr. Lehmann's labors. Every psychologist who is interested in the relations of his science to the problems of the history of culture and of the development of rationality, is sure to find in this volume an almost indispensable aid.

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De la méthode dans la psychologie des sentiments. F. RAUH. Paris, Alcan. 1899. Pp. 305.

The special theme of this volume is worked out with reference to its general bearing on the questions of psychological method. What makes psychology scientific? That, broadly stated, is the problem. And the answer given is, Not a particular method, but an attitude. The scientific attitude is characterized by submission to facts. A science is made up of facts and laws and of theories. But the theories here have relation only to prevision of facts or to coördination of the laws of prevision. In either case they are in close touch with experience. A scientific theory, therefore, is very different from a philosophical. The latter seeks to satisfy the demand for unity and system. The scientific spirit is opposed to the spirit of system in that it adapts itself to immediate experience. Judging by these tests, the author finds much in current psychology to criticise. It aims, he thinks, too directly at unity, and achieves an elegant schematism; it makes hasty generalizations and employs conceptions that are too abstract and simple; it relies too exclusively on particular methods, instead of freely using all methods, each in turn according to circumstances; it fails to

do justice to the variety and individuality of the particular instances and hence overlooks the necessity of constantly correcting by new experience the views, rather than theories, to which the spectacle of the mental life gives rise.

Illustrations for these criticisms are drawn in the first instance from assumptions of the physiological school. The author selects especially three: (1) it is wrong to speak of the reciprocal influence of the physical and the psychical—the psychophysical fact is indivisible; (2) psychological problems must be attacked from beneath, by advancing to the complex from the simple; and (3) the reflex act is the fundamental fact of the nervous system, and consequently of psychology. He tries to show that these assumptions are all scientifically worthless. He cites Richet, Binet and Ribot as abandoning the intransigent attitude of their earlier works, but the school, he says, has too long imagined that the business of the psychologist is to translate into the language of a hypothetical physiology the data of a commonplace observation. Similar onesidedness is found in the exaggerated importance attached to experimental psychology in the laboratory. The author notes two special dangers in this form of research, namely that its observations, if impersonal, are insignificant, while if they were personal and interesting, they are irremediably variable. He remarks too on the striking disproportion between the labor and ingenuity expended and the poverty of the results. He commends the method, but sees in it no ground for disparaging the large investigations made in the great laboratory of life. "All the same," he says, "our first advice to a student of psychology would be to read novels and go into the world." He finds a like overvaluation, in individual psychology, of the method of the questionnaire relatively to the judicious use of autobiography and memoirs. Finally, as representing a different type of thought, he instances the dynamic, teleological psychology of Paulhan. He regards this point of view as useful to oppose to absolute mechanism, but not any more scientific.

All this is preliminary to the detailed demonstration, in the psychology of the sentiments, of the insufficiency, and also of the relative value, of any one method or theory. The subject is approached, in the second chapter, by a series of definitions. Sentiments (feelings) are defined as subjective, individual facts of consciousness, as opposed to images and thoughts, the objective facts of knowledge. They are also defined as forces acting within the limits of a determinate body. From this latter point of view every fact of consciousness can be regarded as a sentiment. The distinction is drawn be-

tween conscious and unconscious, or 'real,' sentiments, and again between sentiments as tendencies and as mental states. The term tendency is used to cover continuity of process as well as direction towards an end. After much that is fine and discriminating, it is something of a surprise to find emotions described as affective states attaining a certain degree of consciousness and comprising pleasures and pains, and to see the passions defined in no other terms than as the tendencies that arise in consequence of a felt or imagined pleasure or pain. The three succeeding chapters develop, with critical reference to opposing views, the principal distinctions given above. Then follows, in the sixth chapter, a classification of theories. These relate to the nature of the sentiments and to methods of treatment. The sentiments can be regarded as intellectual, as organic, or as special facts, and they can be treated by the physico-mechanical, by the physico-chemical, or by the biological method. As a fourth, yet common, method may be added the classification preliminary to any investigation. The distinction drawn between the physico-mechanical and the physico-chemical methods is that the former treats the sentiments as measurable forces in mechanical relation, while the latter treats them in their relations of succession and coexistence, but apart from any idea of teleology. The rest of the work contains an elaborate investigation of these views and methods. The thesis sought to be established is that, whatever the nature of the psychological theories relative to the sentiments, there is no one which is absolutely to be preferred, but that such theories are modes of explanation verifiable or simply utilizable within restricted limits (p. 106). And the general conclusion is, that a psychology should be modest, free, supple, undulating (*ondoyante*), not very abstract and not at all pedantic (p. 303).

With these results the majority of American psychologists, at least, will probably heartily agree. But though they will read with complacency the complimentary references which M. Rauh makes to the breadth of view of a number of them, it may not be without profit even here to ponder anew the lessons of his book. He possibly exaggerates the distance which separates a philosophical theory from a scientific and underrates the value for science itself of the attempt to carry through systematically a chosen methodological points of view. Nor is he perhaps just at all points to the authors he criticises. He treats, for example, Paulhan's functional teleology as though it entirely disregarded the mechanical aspects of consciousness (p. 23) and James' 'stream of consciousness' as though it did not

itself recognize the difference between transitive and substantive states (Chapter IV.). But the book is rich in psychological observations and methodological suggestions, though at times so subtle as to be obscure.

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Beiträge zur Akustik- und Musikwissenschaft. Herausg. von C. STUMPF. 1 Heft, pp. vi + 108; 2 Heft, pp. 170. Leipzig, Barth, 1898.

Instead of the two volumes which would complete the *Tonpsychologie,* Professor Stumpf has adopted the more practical plan of publishing a series of monographs which are to contain his own researches and the work of other investigators on acoustics and the theory of music. These subjects will be treated from every point of view except the purely physical and the purely historical. The *Beiträge* have thus a wider scope than the author had proposed in his well-known work, and will interest a larger class of readers.

Heft 1 brings us a lengthy paper by Stumpf himself, entitled 'Konsonanz und Dissonanz,' in which he develops some of the fundamental ideas of the *Tonpsychologie.* The definitions given by Helmholtz, Lipps, Oettingen and Riemann are discussed and rejected. The difference between consonant and dissonant tones lies neither in unconscious functions nor in conscious feeling, neither in overtones nor in beats, but in the fusion of the tones themselves. This conclusion is entirely in keeping with the earlier views of Stumpf, and is now, in the main, widely accepted by psychologists. Discussion, in fact, has already raised the question: What, then, is fusion? And Stumpf faces it squarely.

Fusion does not consist in what the elder psychology termed the 'unity of consciousness.' It is not merely non-discrimination of simultaneous tones. All that we can say is that it is 'Einheitlichkeit'—the approach of the components to unison. Strictly speaking, we cannot define it any more than we can define blue or green. We may, however, investigate the causes or conditions upon which fusion depends. The only psychological factor worth considering is tone-similarity. But this, in the octave, is the effect, not the cause, of fusion; and in the other intervals it is altogether wanting. We are rather obliged to admit two fundamental relations: similarity, dependent upon the difference between vibration-rates, and fusion, dependent upon their ratio.

For the physiological explanation, we have to assume that a closer

combination of central processes corresponds to tones whose vibration-ratio is relatively simple. For this assumption we have precedents in other departments of brain-physiology. But when we attempt to go beyond this and arrange the details, we enter the region of hypothesis which is still obscure.

Stumpf's theory, however, is set in clearer light by the definitions he proposes. Tone-combinations which show a higher degree of fusion are consonant; those which show only a low degree of fusion are dissonant. The absolute distance that separates two tones does not of itself constitute a musical interval. To define this, we need the idea of tone-relationship. The interval is that relation between two tones which is determined primarily by their relationship and secondarily by their relative distance from each other. Increased intervals do not, as Helmholtz maintained, either gain or lose in consonance; the addition of an octave leaves both fusion and consonance as they were in the original interval.

The dualistic theory, according to which consonance depends upon the reference of the actually combining tones to a common fundamental or a common overtone, is rejected by Stumpf after a thorough criticism. Psychologically, it is unacceptable, because experience gives no evidence of any such reference or 'representation.' It is furthermore in opposition to our musical consciousness and is beset with inconsistencies.

Stumpf's own position is somewhat more fully explained in the article with which Heft 2 opens: *Neueres über Tonverschmelzung*. The laws of fusion which he had previously formulated and which he here defends against recent criticism, are these: fusion depends upon vibration-ratios; it is independent of the absolute and the relative strength of the component tones; it is not affected by the addition of other tones; it recurs in equal degree when the intervals extend over several octaves. If objections have been raised especially in regard to the last-named law, this is because of the natural tendency to pronounce the fusion of tones less perfect when their distance is greater and when their intensities vary. The removal of these and similar difficulties is largely a question of method. Both direct observation by trained subjects and collective experiments with untrained subjects, are admissible, provided the requisite precautions are taken.

A more detailed discussion of methods is presented by Max Meyer in his paper *Ueber die Unterschiedsempfindlichkeit für Tonhöhen*. The method of just-discernible differences, which Luft employed, is open to the objection that variations other than those in pitch influ-

ence the observer's judgment. Meyer's results, however, obtained by the method of true and false cases, agree in the main with Luft's, and go to show that, in the mid-region of the scale, the discriminative sensibility is independent of the pitch.

The same writer has a contribution on the theory of differential tones and of auditory sensations in general. It is a modification and development of views that he had previously advanced. The resonance theory is abandoned and the existence of resonators in the ear is set down as impossible, inasmuch as it conflicts with evidence furnished by anatomy and pathology. In its stead Meyer proposes an explanation based on an analysis of the tone-wave in the ear, which takes place in such a way that the effect of the smallest excursions is first lost and then gradually the larger die out. Greater amplitude of the air-wave means a larger vibration of the stirrup and consequently the displacement of more fluid. A longer portion of the basilar membrane is thus thrown into motion, and a larger number of nerve-terminals is excited. On this basis Meyer draws out curves representing different intervals, and calculates the extension of the membrane that corresponds to the resulting tones.

The most important of these *Beiträge* is the paper by Stumpf and Meyer on *Maassbestimmungen ueber die Reinheit consonanter Intervalle.* The experiments were made with musically trained observers, and the method of true and false cases was employed throughout. In the first series of experiments there appeared a tendency to judge the minor third 'pure' when the physically pure interval was somewhat reduced. Subsequently it was found that a considerable enlargement of the major third is needed to call out the judgment 'pure'; and the same result was obtained, in Stumpf's own case, from the experiments then made with the minor third. This would indicate a sort of 'Umstimmung' due to the fact that, in the later experiments, the observer had become accustomed to the major third. The contrast, however, says Stumpf, is one not of sensation but of feeling; and its influence varies with individual observers.

Further investigation yielded these results: the tendency to enlarge the interval increases from the major third to the fifth and from this to the octave; the order in which the tones are given does not materially affect the judgment of an interval as to purity; erroneous judgments occur more frequently when the tones are simultaneous than when they are consecutive; overtones affect the judgment unfavorably; sensibility to change in the purity of the interval is about the same for the third, the fifth and the octave, *i. e.*, equal variations

from the subjectively pure interval are in all three cases detected with equal certainty. If, on the other hand, this sensibility is measured by the exactness with which objectively pure intervals are judged, the results conflict decidedly with the traditional notion that the sensibility varies according to the degree of consonance.

The discussion of these results leads to a conclusion which Stumpf considers the most important outcome of the investigation. They are to be explained, he says, not by muscular sensations nor by tone-distances, nor even by fusion in various degrees, but by feelings which, according as they are positive or negative, enable us to say whether an interval is, or is not, pure. These feelings, developed in the course of individual experience, are subject to modification; the subjective 'purity-point' may shift; but they do not differ in quality for different intervals. They are the cause, not the consequence, of our judgment. Originally, they were based on the perception of variations from the normal degree of fusion; then, strengthened by association, they were so bound up with the sensory process that they are now aroused directly by the tone-impressions. At any rate, the feelings are there; and their behavior accounts for the peculiarities which have been noted in judging the purity of intervals.

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Psychologische Untersuchungen über das Lesen auf experimentellen Grundlage. B. ERDMANN and R. M. DODGE. Niemeyer, Halle a/S. 1898. Pp. viii+360.

Intelligent reading, we are told by the authors, consists of several processes. These are always closely bound together, but for convenience of discussion may be divided into the three following groups:

I. The optical perception of the material signs.

II. The reproduction of the sounds that are associated with the perceived words.

III. The reproduction of the meaning.

Of these groups the first two are common to all kinds of reading where signs occur and are read (benennen). These two conditions are present in the reading of letters and words as well as sentences while the third factor is brought into play only when words are considered in sentence connection.

The problem of Professors Erdmann and Dodge is the consideration of the time relations of the first two groups; the conditions of optical perceptions of the printed symbols—letters, words, etc., and the reproduction of the associated sounds.

The early experimental investigations in these lines are discussed and dismissed as incomplete in execution and unsatisfactory in interpretation. Previously the subject had been considered from two separate standpoints, the psychophysiological and the psychopathological. The results of the psychophysiological researches led investigators to believe that in normal reading the reading unit is the word, not the separate letters forming the word. The pathologists, familiar with the varied cases of partial aphasia, considered the letter as the unit. The psychophysiological experiments (*e. g.*, those of Cattell in *Philos. Stud.* and *Mind*) were conducted, we are told, without reference to the different forms of aphasic disturbances, and the pathological theorists (*e. g.*, Grashey, in *Archiv f. Psychiatrie*) considered 'unsystematic data from the field of the physiology of sense perception' to the exclusion of the 'special results that psychophysiological research had already obtained.' A later attempt to combine both points of view (Goldscheider and R. F. Müller, *Ztsch. f. klin. Medizin*) the authors regard as unsuccessful. In method and conclusion the present authors would probably be classed with psychophysiological investigators. Words, they conclude, are optical wholes, but spoken reproductions of these words are letter-combinations. However, it is the opinion of the reviewer that neither by experiment nor by critical review of previous work have the authors explained from their point of view all the partial aphasias that led pathologists to consider letters the basis of the reading process.

The number of letters that can be read at one time is the first problem investigated by the authors. To determine this two methods were employed: (1) the *Spiegel-* and (2) the *Tachistoskopischer-versuch.* From a series of suitable mirror experiments, in which the eye could be observed by an assistant, the authors find that if a printed line is read and the head meantime kept fixed there is a regular change between eye movement and rest (Ruhepausen). The number of these changes is much less than the number of letters in a line, for a line 8.3 cm. *Do* had about five rest periods, and *E* for a 12.2 cm. line took seven rests. The number of the movement and rest period changes with the character of the context, being greater for unfamiliar than for familiar texts. The time necessary for reading familiar passages in the mother tongue and in a foreign language is less than for unfamiliar portions, and the times for reading familiar and unfamiliar portions in the mother tongue are less than those for similar portions of a foreign language. The rest periods, which are taken up with perception, interpretation, etc., are calculated to be from 12 to 20 times

longer than the time of movement. The latter results were not obtained directly, but were determined from other measurements. A much more satisfactory and an unquestioned result would have been obtained had the authors used a method similar to those devised by Delabarre and by Huey.

The field of perception (erkennen) was found to be greater than the field of distinct vision with eye fixed. This result was determined readily by dividing any line into the number of parts corresponding to the eye changes occurring during reading the line.

The reading of single words and of letters by *tachistoskopischer versuch* is next considered. These *Redeteile* should be shown for such time that they are seen as a whole, as a unit. Such a condition may be obtained only by exposing the letter or word sufficiently long to be perceived, but the exposure should be so short that the eye has no time to change its position. This time, the authors say, should be less than the time of a voluntary eye movement (reaction) over an angular distance of five to ten degrees (fovea to blind spot). The time for the eye's reaction is about 0.2 second and the authors adopt 0.1 second for all exposures of words, letters, etc. With this exposure three observers read correctly four and often five simultaneously shown letters. Four to five times as many letters could be read if combined into words. Some of this great difference the authors admit may be due to inability to hold in memory (and reproduce) more than five or six disconnected letters. Other experiments are said to show that words of characteristic form are more easily perceived than those of similar configuration, and words whose optical forms are more familiar to the reader (*muttersprachliche*) are more easily perceived than those which are less familiar (*fremdsprachliche*), and words in sentences are recognized not only by form, but also by grammatical and meaning relations.

The process of speaking a word the authors find not so simple as the perception of a printed word. The visual impression was found to be a simple simultaneous whole. The spoken reproduction of this visual impression is a successive acoustic-motor whole. Thus, the word VASE is a visual whole—VASE—but the spoken reproduction is a successive acoustic-motor whole—V-A-S-E. "The spoken word is a whole only in its combination of acoustic and of motor sensations corresponding part to part."

Following this analysis appears an account of reaction experiments by which the authors' hypothesis seems to be strengthened. The new work is preceded by an extended critique of the reaction

times made by Cattell (*Philos. Stud.*, III.). Some of the results of the authors are: (1) the reaction time to a letter and to a word of four letters is about double that to eight; (2) the time of speech reaction to one of twenty-six four-letter words is somewhat shorter than the time to a single letter; (3) the time of speech reaction to 4-, 8-, 12-, and 16-lettered words is in direct relation to the length of the words.

A chapter is devoted to the description of apparatus for the isolation of reading-pauses and in a final *Anhang* are described experiments determining the speed of eye movements according to the angular distance of movement.

A careful perusal of the book shows in parts carelessness in the preparation of the manuscript or of the proof. It is unfortunate that the volume, in many respects excellent, should be marred by so many typographical errors and errors in calculation (*e. g.*, pp. 86 and 87, column 10, Table VI. should have average 0.55 instead of 0.56 and columns 6 and 8 of Table VII. should have averages 0.09 and 0.31 instead of 0.1 and 0.28). Several conclusions are drawn from experiments the averages of which are given without noting the number of experiments (*e. g.*, Table III., page 58; reaction time averages, pp. 282, 283, etc.), and what is more to be regretted cases were found where conclusions are drawn from insufficient data (*e. g.*, an experiment in which twenty-six single German words were shown to *Do* [an American] and twenty-two to *E* [a German], each subject read twelve words correctly and the authors conclude that "the relation [right: wrong] in which the words for *Do* and for *E* stand to each other [6:7, 6:5] shows the apperceptive influence of greater familiarity with the word forms" [p. 158]. As a matter of fact it is probable that greater familiarity with certain words would permit their being more often perceived, but such a conclusion is unjustified from the results of this experiment).

Although all these errors detract from the value of the book as a reference for results, the volume will undoubtedly fulfil a mission of stimulus and suggestion. An excellent plan has been followed by the authors in numbering serially their conclusions. This makes the book convenient for every one, especially for those that do not care to read details. The reviewer is glad to be able to add that the book is, unlike many German monographs, very readable. The style is clear, the experiments well described and the criticisms fair and moderate.

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L'Année psychologique; 5e année, 1898. Edited by A. BINET. Paris, Schleicher Fr. 1899. Pp. 902.

The *Année psychologique* for 1898 consists as usual of three parts: original articles, analyses of notable books and articles appearing during the year, and a general bibliography. The original section has been gradually enlarged; in the present number it covers 590 pages—about two-thirds of the entire volume—and the contributions embrace a greater variety of topics than formerly, giving a well-rounded character to the periodical. Among the original articles are two important topical reviews, one by V. Henri, on the Muscular Sense, and another by J. Joteyko, on Muscular Fatigue.

Henri finds a recognition of the muscular sense as far back in literature as Descartes (he gives no reference, however), and cites Berkeley, Reid, Erasmus Darwin, Bichat, Steinbuch and Thomas Brown among early writers as having alluded to this sense. To Charles Bell is given the credit of having developed the subject for the first time in a scientific manner, in which he was soon followed by Magendie and later by a number of other investigators. The author divides the researches into physiological, pathological, experimental and theoretic, but discusses only the first three here, reserving the theoretic side for a later paper. His own classification of sensations connected with the muscles is of interest. He finds three chief species: (1) Intrinsic muscular sensations, due to general stimulation of the muscles, tendons and joints, and including the sensations of rest and fatigue. (2) Sensations connected with a condition of muscular immobility; these include the various sensations belonging to specific positions of the motor organs and those due to different degrees of muscular contraction, such as relaxation, involuntary contraction, voluntary contraction, equilibrium of antagonistic muscles, and resistance to movement. (3) Sensations of movement, both passive and active, the latter occurring either with or without resistance. The sensations of movement give rise to perception of the direction, extent, rapidity and duration of the movement and to perception of the intensity of resistance. In connection with passive movement the author discusses the location of the specific end organs for these sensations, and concludes that the evidence indicates the joints as their chief seat, but with certain accessory sensations in the muscles themselves. The article concludes with a bibliography of 391 titles, with notes on the most important works mentioned.

Joteyko discusses the recent work on muscular fatigue. The paper deals almost exclusively, however, with researches using the ergo-

graph and other apparatus giving graphic records. The various forms which have been assigned to the normal fatigue curve are examined, and the author concludes in favor of a curve of three phases: a period of gradually increasing excitability, followed by a period of rapid decrease when fatigue first sets in, and finally one of gradual decrease till complete exhaustion occurs; this exhaustion is to be regarded, however, as nervous rather than muscular. The modifications of the fatigue curve in man due to differences in intensity of stimulus, amount of weight applied, and rhythm of action are discussed, with special reference to the problem of the maximum amount of work procurable. Among other topics surveyed are the influence of the temperature of the muscle, age of the subject and use of drugs in modifying the form of the curve. The periodicity of a curve due to partial neural restoration, and the influence of various conditions of blood circulation and oxidation of muscle on the progress of fatigue and restoration are noted. The bibliography, which is chronologically arranged, contains 98 titles.

Among the original investigations in the *Année* are three devoted to mental work and fatigue. All three approach the subject from the standpoint of physiological effects. This method, it will be observed, has found special favor in France, of recent years, as shown by the number of investigations of this nature published by French authors. In Germany, more direct tests appear to be favored, such as the measurement of the amount of mental work actually performed in equal periods of time, before and during the progress of mental fatigue, the accuracy of such work, etc. It would be interesting to compare the results of these two radically different types of method, with a view to determining their relative merit.

J. Larguier's paper (*Essai de comparaison sur les différentes méthodes proposées pour la mesure de la fatigue intellectuelle*) describes four methods adopted by the author for measuring intellectual fatigue. (1) The relative frequency of the pulse when the subject is at rest and engaged in intellectual labor. (2) Changes in the temperature of the body caused by mental application. (3) Changes in muscular force, as measured by the progress of muscular fatigue. (4) Changes in tactile sensibility, as measured by the æsthesiometer. The author combines the results obtained by these methods in the following statement: "After labor sufficiently prolonged for the subject to be conscious of a certain amount of intellectual fatigue: his tactile sensibility was found to be diminished, but only in certain regions; his muscular strength was increased; his temperature was constantly lowered; and finally, his circulation underwent a distinct modification."

V. Henri (*Influence du travail intellectuel sur les échanges nutritifs*) discusses the influence of habitual intellectual work on the digestion, which he claims to be the most important of all its physiological effects. The experiments by means of this method were necessarily concerned with long periods of mental application—days, not hours—and involved the chemical analysis of the subject's solid and liquid discharges during these periods, which were compared with those occurring during like periods of relative inactivity. The subject adopted a regular diet, constant as to quantity and quality, which was so chosen that the weight of the body did not vary while it continued; the hours for meals and the consumption at each meal were also fixed. The actual experiments were preceded by a period of about a month, during which the exact quantity and nature of food were determined and the subject (the author himself) became accustomed to the diet. Following this was a period of 4 or 5 days during which he performed as little intellectual labor as possible; then a like period during which he performed severe intellectual work, requiring great concentration of the attention, for several hours each day; finally another period of rest, similar to the second. Owing to the lack of an assistant to perform the requisite chemical analyses during the periods of rest, the research was not completed, and the principal results obtained were the determination of a diet suitable for the experiment, which is discussed at some length; of special interest is the observation that rice is the most available vegetable for such a test, since it varies least and its use can be continued for a long period without distaste. The author insists on the importance of analyzing the solid excrements as well as the urine, which most investigators in this field have neglected to do.

A. Binet (*Note relative à l'influence du travail intellectuel sur la consommation du pain dans les écoles*) replies to a criticism by E. Blum of his paper in the last volume of the *Année*, on the influence of intellectual work on the consumption of bread among school children. Professor Binet found a gradual diminution in the consumption of bread during the scholastic year, and assuming the general constancy in amount of other foods used, concluded that prolonged mental work gradually impaired the appetite. M. Blum, as a result of similar investigations of his own, questions both the fact and the deduction. M. Binet maintains that while servants and instructors as well as pupils partook of the bread, this does not vitiate the results; the former were subject to the same influences (meteorological and other) as the latter, except in the matter of intellectual work,

which was therefore the sole variable condition. M. Binet promises more extended statistics on the subject at a later date, however. His method is an interesting one, and his results are of importance if they are sustained by further investigations.

The analytic portion of the *Année* contains fewer articles than usual, but greater care has been exercised in their selection, and they are fairly representative in the departments of Sensation and Pedagogy; the chief works on Hearing are summarized in a single review. The general bibliography, forming the third section, is identical with the PSYCHOLOGICAL INDEX issued by this REVIEW.

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VISUAL PERCEPTION.

Die optische Localisation der Medianebene. M. SACHS und R. WLASSAK. Zeitsch. f. Psych. u. Phys., XXII., I., pp. 23–46.

This paper reports a series of experiments in which a subject seated in a dark room was shown a narrow vertical slit uniformly illuminated from behind, and was required to judge, from various positions of the head and eyes, whether the slit was straight ahead or on the right or left side. In presenting the results the authors draw a distinction which is of cardinal importance for their theoretical interpretations. It is the distinction between correctness (*Richtigkeit*) and certainty (*Bestimmtheit*) of judgment. Correctness means the degree of correspondence between the objective median plane of the body and the average judgments of the subject. Certainty means the narrowness of the area in which the judgments right and left may overlap, since the narrower this area the more closely defined is the judgment, 'straight ahead.'

Correctness is greatest when the head is in its normal position, *i. e.*, with its median plane coincident with that of the trunk, and when the eyes are left free to fixate where they will. When one eye is covered, the correctness is reduced and varies according to the eye used. When the head is turned, the median plane of the head becomes the determining plane for judgment, the correctness is accordingly small. From this it is argued that the sensations of movement from the neck muscles are of small importance in these judgments.

Certainty of judgment is regarded by the writers as of much greater significance than correctness. The highest degree of certainty found in any of these experiments was obtained by fixating the eyes steadily on

some bright point placed at the right of the median plane, the head being in such cases held in the normal position and the eyes being turned to the fixation point. These were the only conditions in the series of experiments under which the eyes had a definite point of fixation. In all other cases the eyes were left to wander until the slit appeared and then they fixated the slit. The argument of the authors from these results is that the most favorable conditions for judgment are not obtained from free movements of fixation.

Further evidence against the movement hypothesis is found in the fact that when the slit is moved through the field judgment is not so certain as when the slit is shown successively in a number of stationary positions. Also when movement from the lateral point of fixation to the slit is allowed, judgment is less certain than when the eyes are kept fixated on the point.

On these grounds the general conclusion of the whole paper is that sensations of movement do not play any important part in two-dimensional visual localization. Such localization depends rather on the relation of the image to the median axis of the retina. In short, the interpretation is in favor of Hering's theory of visual space perception.

The basis for such a general conclusion does not seem to be at all adequate. The number of results given is very small. The authors offer those presented, 'as examples,' but generally the examples are limited to one set of figures. In a judgment of such complexity certainly one or two cases will not serve as sufficient basis for general inductions. Again, it is not clear what the authors mean by the judgment 'straight ahead.' The position of the head does not, in the ordinary circumstances of life, or in any other experiment with which the reviewer is acquainted, determine our perceptions of the median plane of the trunk in any such way that we are illuded into the belief that the trunk has moved with the head. It is not easy to see, therefore, on what grounds it is argued that the influence of sensation from the neck muscles is unimportant. Finally, the high degree of certainty resulting from fixation of a lateral point is probably not due so much to the positions of the images on the retinas as it is to the fact that under these circumstances, and under these circumstances only, the subject had a definite point of comparison given in the field of vision, whereas in the other cases the localization depends on a much less direct and simple judgment. That such an explanation as this, rather than the one given by the authors, is the true one, would seem to appear also in the fact—which is altogether too easily disposed of in the paper—that correctness and certainty do not follow the same laws. This

being true, it would seem to be obvious that the degree of correctness is the more important datum for generalization in regard to the mode of localization. Certainty merely gives us an indication of the directness or indirectness of such judgments of localization. Any vagueness to be found in any of the cases just described is as likely to result from vagueness of bodily sensations of touch as from uncertainty in movement sensations.

The Perception of Horizontal and of Vertical Lines. Prof. B. O. Peirce. Science, Sept. 29, 1899.

Prof. Peirce reports in this article a large number of observations on the accuracy with which horizontal and vertical visual directions can be judged when all objective lines of reference are removed. The paper is unique in that it confines itself to a statement of the methods employed and the results obtained, no attempt being made to interpret the facts discovered or to establish any general principles of space perception.

The first method of these experiments was to allow the subject to turn the eyepiece of a horizontal telescope containing a single cross hair until the cross hair seemed either horizontal or vertical. A piece of ground glass placed in front of the object glass excluded all disturbing images. The second method used was the following. The observer looked at a surface crossed by a single line, through a tube which was two meters long and 35 centimeters in diameter, was blackened inside and was supplied with several diaphragms to prevent reflection. The line was now placed by the subject in what seemed to be a horizontal or a vertical position and the degree of deviation from the objective horizontal or vertical was recorded.

The results show that children and others who are without training in judgment of direction tend to set the line carelessly, but, after all, average fairly near to the objective standard. Training reduces the amount of variation. Astigmatism, even of a pronounced form, does not affect the judgment. When monocular vision is tested it is found that the line which seems horizontal to the right eye must be rotated slightly counter-clockwise in order to appear horizontal to the left eye. Tipping the chair or platform on which the subject sits or stands seems to confuse the subject somewhat and results in a lower degree of accuracy of the judgment. No constant relation was found between the direction of the tipping and the direction of error in the judgments. Deviations are also greater when the head is turned so as to

look over the shoulder, or when the body is bent so that the subject looks between his legs. When the body is erect and the head is much inclined to the left deviations are constantly counter-clockwise. When the inclination of the head is to the right the deviations are clockwise. The same holds for readings taken when the observer lay on a horizontal shelf on the left or right side. The deviations in such cases sometimes reach as high as 20 and 22 degrees.

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La phonétique expérimentale appliquée à l'enseignement des langues vivantes. Par ADOLPHE ZUND BURGUET. P. 35. Paris. 1898.

Applications pratiques de la phonétique expérimentale. By the same author. P. 24. Paris. 1899.

While these two pamphlets aim to discuss the practical side of experimental phonetics, they give a brief exposition of the present state of this method of research and a concise illustrated description of the appliances peculiar to it. The phonetic elements of a language are regarded as differing in duration, intensity, pitch and quality according to the relations of the various organs of speech to each other.

It was the recognized inability of ordinary observation to discover the exact relations necessary to the production of different sounds that finally led to the invention of the following appliances: (1) an artificial palate by which the relation of the tongue to the hard palate can be determined, (2) *les ampoules exploratrices*, rubber capsules of different sizes, for recording lip and tongue pressure, (3) *les olives nasales*, small bulbs for registering the amount of air discharged from the nose, (4) *l'embouchure*, a receiver for the air discharged through the mouth, (5) *le signal du larynx*, a complicated instrument for detecting the movements of the larynx, (6) *le cadran-indicateur*, an appliance for indicating to the eye and ear the amount of air expelled to produce a given sound. In use, a kymograph and recording tambour are necessary to 2, 3 and 4.

Though these instruments were primarily intended for research, the author proceeds to discuss them as a means of teaching foreign languages to youths and adults or of correcting faulty pronunciation in their native tongue. It is shown by specific cases how a child (who learns his mother tongue by ear) may acquire a faulty pronunciation and his ear habitually ignore the difference between his own speech and the normal. In such cases some other sense must be appealed to and here experimental phonetics steps in. This same habit

of the ear makes it difficult for an adult to learn a new language. In all such cases apparatus may be brought in to give by eye an indirect perception of the required movements.

The author calls attention to some facts of psychological interest. He illustrates how futile it is to tell a person with a defective pronunciation that his palate must be so, tongue thus, etc., and how easy it is to teach him the correct pronunciation by apparatus where his eye can see by a line upon the drum of a kymograph or by the dial of the cadran-indicateur when he has made the correct movements.

So far as the reviewer is aware very little attention has been given to the relations existing between hearing and the imitative movements of speech. Our principal manner of imitating movements is by eye —there are obvious reasons why this is true for animals as well as men—and not by ear; but a sound is imitated from what appears to be very meager data. It is well known that in a movement the kinæsthetic 'copy' is the complex experience of the correct movement, but here we usually have an ever-present objective test, as in striking a certain key on a piano.

In the case of an articulation we have only a remembered sound as the test. It is with some reason, then, that our author says the acquisition of a correct pronunciation is somewhat a matter of chance, for we see that with such an indirect test the wrong kinæsthetic 'copy' may become fixed. Now, the use of the afore-mentioned apparatus in correcting such an erroneous copy is to provide a visible test so that the correct movement may be noted among the random ones and repeated until it becomes fixed in place of the other. In teaching a new articulation it gives a familiar sign of the correctness of a movement whose copy may thus become fixed. The reviewer suggests that these methods may be valuable in teaching the deaf to speak.

The author passes hastily over variations in articulation because he has other points in view, but it seems especially important to know something of the exactness with which words are pronounced. If experimental phonetics is to maintain its claim to an exact method of perpetuating a language, the variations of the individual and of the individual from his fellows must be known. In any case this would be an important research from a psychological point of view.

As a whole, experimental phonetics is a laudable achievement and is one of the more recent triumphs of the exact method, but it is too early to decide upon its value as a method of instruction, when it can at best have only a limited application.

CLARK WISSLER.

The Memory Image and its Qualitative Fidelity. I. Madison Bentley, Ph.D. American Journal of Psychology, XI., 1, October, 1899, pp. 1–48.

The author tries 'to determine the exact place of the image in the memory consciousness' and 'to discover the changes which the image undergoes in the course of time'; to which are prefixed an examination of the place given to the image in the memory literature, and a discussion of the nature, function and genetic significance of the image. His experiments, conducted by the comparison method, consisted in presenting successively two shades of gray, made by a Marbe adjustable color mixer. A norm and two variations were used, one having 5° of arc more white, and the other 5° less than the norm, a difference considered to be about liminal. The subject judged whether the second shade, 5 seconds later, was the same as, or lighter or darker than, the norm. The experiments were carried out under two conditions of illumination. One set was done by diffused daylight and another in the dark room where the rotating disc was the only object affecting the retina. It was found that in the daylight the subjects judged the second stimulus to be darker, when it was the same, or lighter, more often than they did in the dark room. This Dr. Bentley conceives to mean that the memory grows lighter in the daylight and darker in the dark (p. 47), thus showing that the stimulated or unstimulated state of the rest of the retina plays an important part in the quality of the memory image. He finds that 'the tendency of the memory to lighten increases with the length of the interval between stimuli.' Other results are that from 2 to 6 seconds may elapse between stimuli without uniformly affecting the brightness of the memory image; that the infidelity (sic) of the memory image increases from 1 to 5 minutes, but the memory image itself is 'more readily producible after 5 minutes than after 1 minute.' The two methods above mentioned were supplemented by a third, a series of experiments having been performed in which the second stimulus was gradually altered until the subject thought it equal to the first; and three 'subsidiary methods' were tried and found unsatisfactory, viz.: recall and selection (different colored papers twice exposed); aqueous solution of pigment formed by burette droppings, which was found defective as an objective standard; and direct reproduction (the subject mixing and applying a color wash to paper).

It seems curious at first sight that a research nominally upon the qualitative fidelity of the memory image should yet be conducted in such a way that the actual presence of the memory image itself as a

part of consciousness during the period of time occupied by the experiment is not essential. But the fact remains that a record of images was kept showing that in one-sixth of the experiments the subjects had no visual image at all. Their introspection showed, instead, verbal mental content. They helped their memory for brightnesses by saying 'dark gray,' 'light gray,' etc., mentally. One with feminine *Apperceptionsmasse* interpreted the shades into likes and dislikes of gray cloth dress goods. From a comparison of the hundred imageless experiments, and the five hundred experiments that had images, with the accuracy of recall Dr. Bentley infers that an 'imageful recall' is better after a lapse of five minutes than after a lapse of one minute.

In connection with the interpretation and application of the foregoing experiments, two questions present themselves. (1) Does the comparison of two objective stimuli tell anything about the 'qualitative fidelity' of images, particularly when those images are of such a nature that they may or may not be a part of consciousness during the experiment? Is not the research, then, rather a quantitative estimate of retentiveness, especially that part of it concerning the effect of time upon the memory? (2) Why should experiments upon imagery, whether visual or otherwise, be concerned with memory work? To be sure, all images, as representations, are memory images; even the images which are the groundwork of the productions of creative imagination are memory images to the extent of being composed of elements belonging to the subject's past. But though all images may be regarded as memory images, and though by introspection much clearer insight may be gained into the processes of memory, much more will be gained by leaving the memory aspect of images out of account and paying stricter attention to the description of the images themselves and their relations one to another.

WILFRID LAY.

COMPARATIVE PSYCHOLOGY.

On the Instincts and Habits of the Solitary Wasps. G. W. and ELIZABETH G. PECKHAM. Wisconsin Geological and Natural History Survey, Bulletin, No. 2, Madison, Wis., 1898. 245 pp. With plates.

Dürfen wir den Ameisen und Bienen psychische Qualitäten Zuschreiben. ALBRECHT BETHE. Pflüger's Archiv, LXX., 1898. 85 pp.

Studies on Reactions to Stimüli in Unicellular Organisms. H. S. JENNINGS. II. *The Mechanism of the Motor Reactions of Paramecium*, American Journal of Physiology, Vol. II., No. 4. 1899. III. *Reactions to Localized Stimuli in Spirostomum and Stentor*, American Naturalist, Vol. XXXIII., No. 389, 1899. IV. *Laws of Chemotaxis in Paramecium*, American Journal of Physiology, Vol. II., No. 4, 1899. V. *On the Movements and Motor Reflexes of the Flagellata and Ciliata*, American Journal of Physiology, Vol. III., No. 6, 1900. *The Psychology of a Protozoan*, American Journal of Psychology, Vol. X., No. 4, 1899.

Animal Behavior. C. O. WHITMAN. In 'Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl,' Boston, 1899.

Haben die Fische ein Gedächtniss. L. EDINGER. München, 1899. 30 pp. (Reprinted from the Allgemeinen Zeitung.)

Do the Reactions of the Lower Animals against Injury indicate Pain Sensations? W. W. NORMAN. American Journal of Physiology, Vol. III., No. 6.

These titles represent a real advance in comparative psychology. Though dealing with widely different topics and in widely different ways, they have some common features that augur well for the progress of what one hardly dares call, at present, a science. All in common represent work done by scientific workers of ability in other lines, not by amateurs or enthusiasts. All show a critical spirit and an attempt to get beyond superficial observations and beneath verbal quarrels. They seem to promise that comparative psychology will soon cease to be a recreation for naturalists good, bad and indifferent, and a means of speculation for professors of philosophy, and become the serious business of scientific men.

The Peckhams studied the behavior of the solitary wasps, especially their stinging and nest-building activities. Their method was chiefly patient observation of the animals in their natural surroundings, though they made frequent use of artificial conditions. Their important conclusions are that the instinctive activities observed (1) were in the form of coarse, rough-hewn responses to the various situations involved, (2) varied with different individuals, and (3) were modifiable by experience. They deny the validity of Fabre's inference that these insects display wonderful precision and discrimination in stinging in the exact spot required in order to paralyze the victim, and

show what is apparently decisive evidence to support their denial. Their work is reported with great detail and is beautifully illustrated.

Dr. Bethe's general attitude is in sharp contrast with that of the Peckhams and the excellence of his work shows that no single way of studying animal behavior is the golden road. He realizes the broadest issues instead of confining his questions to details; he everywhere attempts decisive experiments instead of relying largely on the results of evidence furnished by the natural course of events; he is a zealous defendant of a certain general theory, for which, one is tempted to say, he seeks evidence.

It will be proper to give only the broader conclusions to which his work leads him, since the evidence should be examined in his own account and since also none but an expert observer of insect life is qualified to criticise it.

Dr. Bethe wisely takes as the evidence of mental functions *modifiability of reactions*. Such modifiability must also be of a degree not to be accounted for by the mere plasticity of all bodily organs. He then shows that animals may be distinguishable by giving off certain substances with different chemical properties to which olfactory organs react. He finds the reactions of ants to so-called 'friends' and 'enemies' to be merely reactions to ants having a different smell from that of those in the nests. 'Enemies' washed and anointed with the excretions of the nest inhabitants are no longer treated as enemies, though of widely different color and size. This reaction, he says, is in no sense acquired or learned or modifiable and does not involve any sort of consciousness. He finds their finding their way to food and to the nest to be due again to a reflex which causes them to follow the trail left by the first accidental passenger, this first passenger following his own trail back. The gradual straightening of such trails is due to the cutting off of complete loops and such straightening of curves as might happen from the automatic following of such a trail. This trail is, however, complicated by being polarized. That is, the trail made by a loaded ant differs from that made by a free ant and the reflex act for a free ant differs from that of a loaded ant. This is his explanation of the fact that an ant picked up, turned around and put on a trail will again resume his old direction, also of the appearance of coöperation shown when one ant going from the nest happens to take the load from a returning ant, both then turning back, the former to the nest, the latter to the source of food. All these assertions are based on direct evidence, though at least one important point is not made thoroughly clear, namely, why the loaded first ant goes back on

a trail which would seem fitted to arouse movement only in the opposite direction in his case. As before Bethe denies the need of any mental factor. "In the case of the ants finding the way does not rest on a mental process. It is rather a complicated but analyzable reflex mechanism."

He also finds no sign of communication or of adaptation to really new circumstances, though his experiments along the latter lines seem to have been few.

Bees like ants react not to 'friends' and 'foes,' but to customary and strange scents. But their success in finding their way seems to be even more of a problem than any other investigators have supposed. Not scents, nor differences in brightness (though both may have some little influence) nor shape, nor size nor the earth's magnetism guide the bee in its movements. Dr. Bethe has tested each of these hypotheses in some cases with extreme ingenuity, and fails to find any one at all satisfactory. Bees come back to the *place* in space from which they started regardless of whether the hive be still there or the surroundings changed or the scent different or they themselves loaded with magnets. 'A to us entirely unknown force' controls the direction of their flight.

Finally bees do not profit by experience or necessarily possess consciousness. Dr. Bethe makes such statements emphatically, though to my mind one fact which he reports suggests that perhaps they may, and he has not perhaps wholly nullified the force of the evidence of Lubbock, Wasmann and others. One respects him however for confessing to 'eine uns ganz unbekannte Kraft' rather than calling in 'Intelligence' as a purely negative and verbal resort.

Dr. Jennings has gained really brilliant results by reason of the extreme thoroughness of his work. By examining the so-called chemotaxis, thermotaxis, thigmotaxis, etc., of several typical microorganisms he has apparently brought order out of chaos and reduced a mass of mystery to a few clear and simple facts. I say apparently because as yet his results are not everywhere accepted. By going to the bottom of the matter and watching the actual behavior of these animals when they 'are attracted by' certain things and 'avoid' others, he has found that they really have only slight modifications of a single accomplishment, a solitary reaction. On increase of stimulation they turn toward one side and shoot ahead again. This may be preceded by a stop, contraction and backward movement. Some forms show also a differentiation in that they go back in the case of a mechanical stimulus only when such stimulus is applied at the anterior part of the body. If applied at the posterior end they go forward.

Apart from these modifications the micro-organisms, studied show no specialized reactions. This one process brings about all the phenomena of congregation in certain spots, etc., described by previous observers, because, random movements bringing the animals into such spots, this single mode of reaction prevents their getting out.

Dr. Jennings' conclusion regarding the amount and sort of consciousness justly ascribable to these protozoa is that no more is needed to account for their behavior than for that of a muscle cell in the human body. He finds no signs of modifiability in the reaction.

Professor Whitman's paper contains a good deal of information valuable for its own sake, but is best considered as an argument against the view that instincts are inherited habits and represent the accumulated remains of individual adaptations. Professor Whitman contends that instinctive acts have the same mode of origin as reflexes, digestion, excretion and all other functions, that they are due to organic features which are the result of germ-variations, and that we should therefore expect to find a continuous development of the instincts of later forms from those of earlier. The evidence which he presents is instructive in the highest degree. The comparative psychologist can no longer rest content with enumerating and describing an animal's instincts; he must now try to follow Professor Whitman and show their evolution, a hard but fascinating task.

Dr. Edinger studied by means of a questionnaire the power of fishes to adapt their conduct to fit novel situations. He decides that they can because of the phenomena of taming and association of the sight of an attendant with the impulse to come for food. He is very chary about believing in any further mental adaptations than these, and seems to allow that fishes without any homologue to the mammalian cortex can have feelings only because he is compelled to. This work is interesting, more because it promises that the comparative neurologists will soon make use of psychological methods than for its immediate conclusions.

Dr. Norman studied the reactions to mutilation of representative animals from the flat-worms to the fishes, and found that they gave no evidence that the animals in question felt pain. The nature of his method and the quality of the work may perhaps be best shown by a quotation.

"If an earthworm be cut in two in the middle, only the posterior half shows the squirming and jerking motions which appear to indicate pain. The anterior half crawls away. * * * It would certainly be very strange if only the posterior half of an earthworm should show

pain symptoms, while the front half, which contains the brain, showed no such reactions. Still more remarkable, however, are the results as one continues with the division of the pieces. Let us designate the front half of the divided worm with *a* and the posterior half *b*. Now if we cut *a* in two in the middle, the front piece, which we will designate as a_1, elongates and creeps forward, while the posterior piece a_2 executes strong jerking and squirming motions. Now if we cut in two the piece *b* (the posterior half of the worm), the front piece b_1 behaves or reacts as *a*, and the posterior piece b_2 as a_2. We now see that each time the whole animal or either half of it is cut in two the posterior piece makes squirming and jerking motions while the front piece makes no motions or reactions that could be considered as indicating pain. * * *"

Dr. Norman goes on to show that as any posterior piece might as well, by a different scheme of cutting, have been a front piece, the squirming, etc., can signify nothing more than a specialized movement in the case of impulses *travelling posteriorly*.

Dr. Norman's patient objective work should serve as a warning, possibly a rebuke, to those psychologists who speculate freely about all sorts of elementary conscious processes in the lower animals without having really any solid knowledge of what their nervous systems as mere mechanisms are capable of, and without ever looking at the facts first hand to find out.

EDWARD THORNDIKE.

NEUROLOGY.

The Nervous System and its Constituent Neurones. Designed for the Use of Practitioners of Medicine and of Students of Medicine and of Psychology. LEWELLYS F. BARKER, M.B., Tor. New York, D. Appleton and Company. 1899.

The considerable importance of this elaborate volume consists, not so much in the opinions of its author which it expresses, in number relatively few, as in the fact that it summarizes just that great body of neurological research in which no modern psychologist can fail to be extremely interested. The labor which this volume represents is suggested by the index of authors and investigators from the products of whose industry it is compiled, for this list contains 700 names more or less, ranging from Acquisito to Zuckerkandl.

The work is divided into six sections (the last of them constituting two-thirds of the book), the titles of which are as follows: The History

of the Development of the Neurone Concept; The External Morphology of Neurones; The Internal Morphology of Neurones; The Histogenetic Relations of the Neurones; The Neurone as the Unit in Physiological and Pathological Processes; and On the Grouping and Chaining Together of Neurones in a Complex Nervous System like that of Man and [the] Higher Animals.

The first section deals with the neurone concept, as derived from the researches of His, Golgi, Forel, and Ramón y Cajal, gives at some length due credit to the staining discoveries of Golgi (especially that of the use of silver impregnations), and discusses interestingly the origin and employment of the term neurone. Held's scientific contributions regarding the anatomical relation of neurones, when properly viewed, he considers confirmatory rather than the contrary of the neurone theory. He thinks Apáthy's discoveries far less revolutionary than some consider them, and, on the whole, advises a degree of scepticism for the present regarding his 'deductions and hypotheses'; it is, he says, 'altogether too early, however, to pass judgment upon Apáthy's views,' although he appears to argue strongly against that now eminent worker's notion that some nerve cells are solely trophic in function. The great service of Waldemeyer is emphasized (he having, in 1891, brought all the converging neuronological views to a focus for the first time) a service, by the way, which Dr. Barker has in this very book almost duplicated. The neurone concept is summed up in little thus: "The nervous system, aside from its neuroglia, ependymal cells, blood-vessels, and lymphatics, consists of an enormous number of individual elements or neurones. Each neurone in its entirety represents a single body cell. These units are at first *entirely* (if protoplasmic bridges be excepted) and continue throughout life *relatively* to be morphologically, and in part, at least, physiologically independent of one another." He thinks there is no sufficient evidence of a 'diffuse neural network.'

Section second is devoted to the general appearance of the neurones as regards their external shapes and relations, but can have no detailed abstract here. The author notes with obvious great interest the very recent notion of Nissl (1898) regarding an intercellular substance as making the difference between the gray matter and the white. In closing this section of his book, its author takes occasion to express surprise, not too well based, at the mass of hypotheses as to sleep, anæsthesia, hysteria, and double consciousness which have grown up about the 'feebly supported' retraction theory of the dendrites.

The third section, discussing the internal structure of the neural

element, is summarized in its last, the fourteenth, chapter, and especially, as is stated, in the following paragraph: "A neurone is made up, like all other cells, of nucleus and protoplasm. In the latter a centrosome and a so-called attraction sphere are present; at least they have been demonstrated in a certain number of nerve cells. The protoplasmic portion of the cell can be roughly divided into a peripheral exoplasmic portion and a central endoplasmic portion. In neurones, as in muscle cells, though less distinct in the former than in the latter, there is a tendency to a fibrillary structure, the fibrillation being more pronounced in the peripheral exoplasmic portion of both nerve and muscle cells than in the endoplasmic portion of the protoplasm. In both exoplasm and endoplasm there can be made out, in tissues which have been fixed, a more or less homogeneous ground substance in which are deposited larger and smaller masses of a granular nature."

Section fourth, treating of the 'histogenetic relations of the neurones,' need not here receive even brief review, it being almost wholly embryological. The genetic psychologist will find the chapters of this part, however, of no little suggestiveness.

The fifth section, that perhaps on the whole of greatest interest to students of animal life's mental aspect, discusses, after an introductory chapter, the degeneration and regeneration of neurones; their irritability; the neurone as a unit of nervous system; the direction of conduction in neurones and the theory of their dynamic polarity; the relations of trophic to nervous functions in the neurone; and the histological alterations in neurones due to the action of poisons and the comparison of these with the effects of traumatism.

The last section of this work, devoted to the relations which neurologists have thought the neurones bear to each other, constitutes 771 pages and has a table of contents of its own. Both the peripheral and the central connections of the cells are exhaustively and elaborately discussed from the view-points of many investigators. Its wealth of detailed fact and more or less well-supported theory is, however, very far beyond the limits of reviewing here. The best of readers will find it a maze he cannot, if he would, unravel; time will do it better.

The volume itself is an excellent example of modern typography and illustration, and is well printed on good, though of necessity, thin paper. It consists of xxxii + 1120 pages, and about these are arranged 676 generally excellent illustrations, some of them in colors like the stains, taken from very many recent sources. No pains have

been spared evidently to make these of the greatest possible use to the readers of the book. Besides the illustrations in the text three lithographic plates in colors ornament the volume. Two indices, one of authors and the other of subjects, add still further to the unusual value of this work of Dr. Barker.

On the Bases and Possibilities of a Scientific Psychology and Classification in Mental Disease. W. Lloyd Andriezen, M.D. Jour. Mental Science, Vol. XLV., No. 189. April, 1899. Pp. 257–290.

This interesting article has value not alone because it portrays an aspect of contemporary psychology as seen by a competent medical man and neurologist, but because in addition it suggests advance along a line where it is most needed to-day. In particular it offers a neurological sketch of psychogenesis which is suggestive for every psychologist.

With consciousness in the abstract, because transcendental, Dr. Andriezen has here nothing to do, it being a topic of metaphysics. In the widest sense consciousness is an endowment of the entire living organism; in the stricter sense, of the nervous system, particularly of the brain. Four methods of studying psychology are mentioned: self-observation, observation on our normal fellows, 'special study, observations, life history and collateral investigation of those with deranged minds,' and, lastly, child-study.

Observation of the very lowest animal orders shows feeling as the sole aspect of mind. The encephalon, developing as morphologic complexity increases, is a recipient organ for various sorts of sensorial stimuli coming to it by various specific pathways: (*a*) various special sense-organs; (*b*) 'kinæsthetic pathways from various movement organs:' the end-organs involved being the muscle-spindles of Kühne; the branching tendon-organs of Golgi; the spiral fibers surrounding certain muscular fibers; and the Pacinian corpuscles of fascia, tendon, and muscular septa; (*c*) by incoming tracts from the viscera.

A classification of the vertebrates is offered based on the constitution of the encephalon, and further, tentatively, according to the proportional extent of the development of the polymorphic layer of the cortex cerebri (the fractions here varying from $\frac{1}{6}$ in rodents to $\frac{1}{3}$ or $\frac{1}{2}$ in man). Because of its possibilities of usefulness this mode of classification deserves some attention.

Dr. Andriezen states that the 'law of psychogenesis' is as follows: "development takes place not only in the sensorial and kinæsthetic

centers, but in the psychical (relational) sphere which combines these two and follows definite lines; and further that during these stages the child is peculiarly impressionable to surroundings."

In new-born mammals while individual nerve-cells have already reached their full numerical growth, the individual nerve-cell in its entirety has by no means reached the acme of its evolution. This continues first, in the size of the cell-body and of its processes, and, secondly, in the extent, variety, and complexity of its connections. Certain other changes of a structural and chemico-physical nature also take place in cell protoplasm and nucleus. Qualitative evolution "comprises growth in the extent and complexity of the connections of the protoplasmic and nervous processes of the brain-cell." These are the important developmental changes as shown by the author's research on young cats by the Golgi method.

Education is suggestively considered from this biological point of view, and the development of the *ego* briefly traced. He distinguishes four stages in the development of the human brain: (*a*) the vegetative somnolescent (intrauterine); (*b*) the immanently sensuous and non-rational stage (the first months of life); (*c*) the primitive rational stage (up to end of the second year); and (*d*) stage of self-particularizing and integration of the *ego* (from the third year onwards). The restraining or ethical nature develops last, and is in a sense represented by the over-arching of the frontal lobes.

"The doctrine of the localization of functions in a central (nervous) organ is the basis which renders a study of psychology and psycho-pathology possible; such a central organ necessarily uniting within itself sensorial, kinæsthetic, and cœnæsthetic functions." The capacities which simple protoplasmic organisms have of sensibility and of response in definite ways constitutes a 'feeling.' "The 'law of pathogenesis' is that many of these evolutions in brain and mind, from the fœtal (vegetative somnolent) to the adolescent (sexually mature) stage, may undergo morbid alteration (perversion) or suffer defect (absence) and such pathological conditions of the brain are the bases of the insanities."

The references at the end of the article constitute a small but useful, because up-to-date, bibliography.

Psychotherapy; or Suggestion as a Cause and Cure of Disease H. H. Seelye, M.D. Medical Record, Vol. 56, No. 10. Sept. 2, 1899. Pp. 325–330.

In this timely and comprehensive address by a competent medical

man (a vice-president of the Massachusetts Medical Society), we have both a simple discussion of the philosophy of psychotherapy and precise evidence of the influence of the mind over the organic tissues as well as functions. Dr. Seelye attributes to suggestive influence on the subconscious self "the many authentic cures of both functional and chronic organic disorders wrought by homœopathists, osteopathists, hydropathists, electropathists, masseurs, faith curers, Christian scientists, religious enthusiasts, blue-glass cranks and by all the other fads that from time to time rage and flourish successfully for a while, only to give way at last to some other new and wonderful method of cure."

He describes at length eight medical cases, as examples, which exhibit either a marked influence of mind on body, or of suggestion, on both without actual hypnosis in any case. It is not often that an eminent physician gives publicly such unequivocal evidence for the control of body-tissues by the mind.

It would be very good for the public well-being could this article, so illustrative of the large field for so-called 'therapeutic' charlatanry, be scattered broadcast through the land.

GEORGE V. N. DEARBORN.

HARVARD UNIVERSITY.

Social Laws, an Outline of Sociology. By G. TARDE, translated from the French by HOWARD C. WARREN, with a preface by J. M. BALDWIN. New York, The Macmillan Co., 1899.

Professor H. C. Warren, of Princeton, has given us an admirable translation of that most suggestive book by M. Tarde 'Les Lois Sociales' which appeared in the Alcan Series in 1898. This is the first work of the French sociologist to be translated into English. It is a condensed statement of the leading ideas embodied in M. Tarde's larger works, namely, 'Les Lois de l'Imitation,' 'La Logique Sociale,' 'L'Opposition Universelle.' But it is rather more than a résumé. It aims at giving the internal bond that unites the three principal works of the French philosopher. Those are, as the author himself puts it, '*disjecta membra* of a single body of ideas' (preface). The unity of the conception may possibly escape the reader's notice when he considers each of the three works separately. In this little book, however, the intimate link between the philosopher's scattered ideas is brought forward with great clearness in the light of a general conception of science, previously but vaguely intimated by M. Tarde and only in the present work sharply defined and presented systematically in its

organic connections. According to M. Tarde, all science subsists 'on similarities, contrasts and harmonies,' that is, on repetitions, oppositions and adaptations (p. 202). Science seeks the laws that govern the repetition, opposition and adaptation of phenomena. In Biology, for example, the tendency of species to multiply in geometric progression (a law of repetition) forms the basis of the struggle for existence, and natural selection (a law of opposition) and the appearance of individual variations, the production of various individual aptitudes and harmonies and the correlation of parts in growth (laws of adaptation) are necessary to the proper functioning of both (pp. 7, 8). In sociology, likewise, we have to study the social phenomenon under three aspects corresponding, respectively, to the repetitions, oppositions and adaptations which it contains and which are obscured by a mass of variations, dissymmetries and disharmonies. The repetition aspect of social phenomena is furnished by imitation. Their opposition aspect must be sought not, as one might be tempted to think, at first sight, in the relation of two contrary or contradictory individuals, but rather in those logical and teleological duals, those curious combats between thesis and antithesis, between willing and *nilling*, whose stage is the consciousness of the social individual (p. 99). This miniature internal battle which is renewed a million times every moment of a nation's life and which takes place whenever a man hesitates between adopting or rejecting a new pattern offered him, whether in the way of phraseology, ritual, concept, canon of art, conduct, constitutes the infinitely minute and infinitely fruitful opposition that underlies history (pp. 83, 84), the moving principle of the bloodiest wars (p. 204), the germ whence proceed the three great forms of social strife, war, competition and discussion (p. 132). The fundamental social adaptation is some individual 'invention' that is destined to be imitated—for what remains locked up in the mind of its creator has no social value (p. 166). An invention is the felicitous interference of two imitations, occurring first in one single mind, and this harmony, though quite internal in origin, tends not only to externalize itself as it spreads, but also to unite with some other invention, in a logical couple, thanks to this imitative diffusion, and so on, until, by successive complications and harmonizations of the harmonies, the grand collective works of the human mind are constructed—a grammar, a theology, an encyclopædia, a code of laws, a natural or artificial organization of labor, a scheme of æsthetics or a system of ethics (pp. 204, 205). These three terms constitute a circular series which is capable of proceeding on and on without ceasing. It is through imitative repetition that in-

vention, the fundamental social adaptation, spreads and is strengthened and tends, through the encounter of one of its own imitative rays with an imitative ray emanating from some other invention, old or new, either to arouse new struggles, or (perhaps directly, perhaps as a result of these struggles) to yield new and more complex inventions which soon radiate out imitatively in turn, and so on indefinitely (p. 135).

Such are the main points of this extremely suggestive book. Certainly, some of M. Tarde's propositions will not pass unchallenged. His conception of science will undoubtedly be much discussed. The alleged circular series of repetitions, oppositions and adaptations claimed to be the typical subject-matter of scientific investigation is evidently nothing more than a vivid image. We cannot attach to it a deeper significance as M. Tarde would have us do, without transforming logical categories into hypostasies of the most scholastic character. Here M. Tarde shows the persistency of a constructive tendency which has oftentimes dragged his clear and powerful mind far from the *via regia* of facts, into the turbid region of metaphysics. We are fervent admirers of the great French sociologist. But one can believe, as we do, that M. Tarde has made a real discovery when he has shown the elementary social fact to be incidental to the transmission of something internal and mental from an individual to another; and yet, in spite of this assumption, which implies, however, the most vital points of Tarde's sociology, one may well perceive the misleading character of the proposed trichotomy of repetition, opposition and adaptation.

Prof. Warren has taken pains to render into clear and good English the vividly picturesque style of the French writer. His task must have been by no means an easy one. It is a matter of particular gratification to all who believe in the future of sociology that experimental psychologists of Dr. Warren's high standing should become so keenly interested in the popularization of the works of the man to whom, after Spencer, sociology owes most.

The book is brought out in an attractive and readable form. It will undoubtedly have many readers and help to prepare a warm reception to the promised translation of 'Les lois de l'imitation.'

GUSTAVO TOSTI.

NEW YORK CITY.

PHILOSOPHY AND LOGIC.

Essai d'une philosophie nouvelle suggérée par la science. Par Léonce Ribert. Paris, Felix Alcan. 1898. Pp. 562.

The fundamental propositions which the author endeavors to establish are as follows: There is a principle of activity (*virtualité*) which is the source of all phenomena through its conjunction with matter. The universe is infinite in space and time. There is a complete determinism, and no such thing as absolute progress, only a relative and a seeming progress. The destiny of man is the hope of surviving in his works, in the lives of his children and in his general influence upon humanity. The work contains an elaborate review of the chief branches of science with the view of reaching a philosophical interpretation of their results. The author's main contention of a practical nature is his criticism of the pessimistic conclusions which his premises might seem to necessitate, but which, nevertheless, as he insists, are not the logical outcome of a true interpretation of the phenomena and laws of science.

L'Avenir de la philosophie. Esquisse d'une synthèse des connaisances fondée sur l'histoire. Par Henri Berr. Paris, Librarie Hachette et Cie. 1899. Pp. 511.

As indicated by the title, the author endeavors to reach some definite philosophical results by means of the historical method. This method consists of a detailed study of the different schools of philosophical opinion, in which the evident conflict of ideas gives rise to doubt, and doubt in turn leads to a criticism which reveals a final precipitate of truth. He passes in review the history of philosophy during the seventeenth, eighteenth and nineteenth centuries; his conclusion is that both idealism and materialism have been discredited, and that the true hypothesis is monism. This is a working hypothesis only and has not been and can not be demonstrated. The future of philosophy is to be characterized by what he calls the reign of synthesis. There is to be progress because there has already been progress. By synthesis, the results of science will be unified; there will be a unity of science and of life, of speculation and of practical activities; there will be a moral order, and a true religion of humanity. The author's point of view is that essentially of positivism; there is, however, an unmistakable tendency to indulge in certain metaphysical speculations at points where the conclusions of positivism do not seem to satisfy him.

Critériologie Générale ou Théorie Générale de la Certitude. Par DR. MERCIER. Paris, Felix Alcan. 1899. Pp. 371.

The author gives a careful historical survey of the general theory of certitude; his criticism is sympathetic and fair. As to his own position, he holds that there is an objective validity attaching to our judgments, inasmuch as in the judgments which seem to carry with them a degree of certitude, the attributive relation of predicate to subject is indicated by the necessary belonging of the predicate to the subject. Therefore the predicate participates in the objective reality which originally pertained to the subject. The predicate being necessitated by the subject, and the subject in turn being unintelligible without the predicate, form therefore evident ground for certitude.

He defines certitude as a psychological reality independent of the causes which give rise to it. It is the complete accord of intelligence with an object as known. There is in certitude an element of repose which is its characteristic feature throughout the manifold forms which it may assume.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

NEW BOOKS.

The World and the Individual. JOSIAH ROYCE. Gifford Lectures before the University of Aberdeen. New York, The Macmillan Co., 1900. Pp. xvi + 588. $3.00.

Notes on the Development of a Child. MILICENT WASHBURN SHINN. Berkeley, published by the University. 1899. Parts II. and IV. Pp. 179–424.

Des Indes à la Planète Mars; étude sur un cas de somnambulisme avec glossolalie. TH. FLOURNOY. Paris, Alcan. 1900. Pp. xii + 420. 8 fr.

Essays on the Foundation of Education. J. GODRYCZ. Lansing, Mich., Lawrence and Van Buren Printing Co. 1900. Pp. 168.

La Morale d'un égoiste. H. LAPLAIGNE. Paris, V. Giard & E. Brière. 1900. Pp. 259.

National Educational Association. Journal of Proceedings and Addresses of the Thirty-eighth Annual Meeting held at Los Angeles, Cal., July 11–14, 1899. Published by the Association, 1899. Pp. 1258.

Les Agnoscies la cécité psychique en particulier. V. NODET. Paris, Alcan. 1899. Pp. 22.

NOTES.

We regret to record the death on February 2d of Dr. Leon M. Solomons, instructor of psychology in the University of Nebraska.

M. Th. Ribot, professor of psychology in the Collège de France, has been elected to the chair of the Paris Academy of Moral Sciences made vacant by the death of M. Nourrisson.

The New Year's honors annually conferred in Great Britain include a peerage for Sir John Lubbock, who will hereafter be known as Lord Avebury, a knighthood for Dr. T. Lauder Brunton, the physiologist, and a K. C. B. for Captain W. de W. Abney, whose researches on color vision are well known to psychologists.

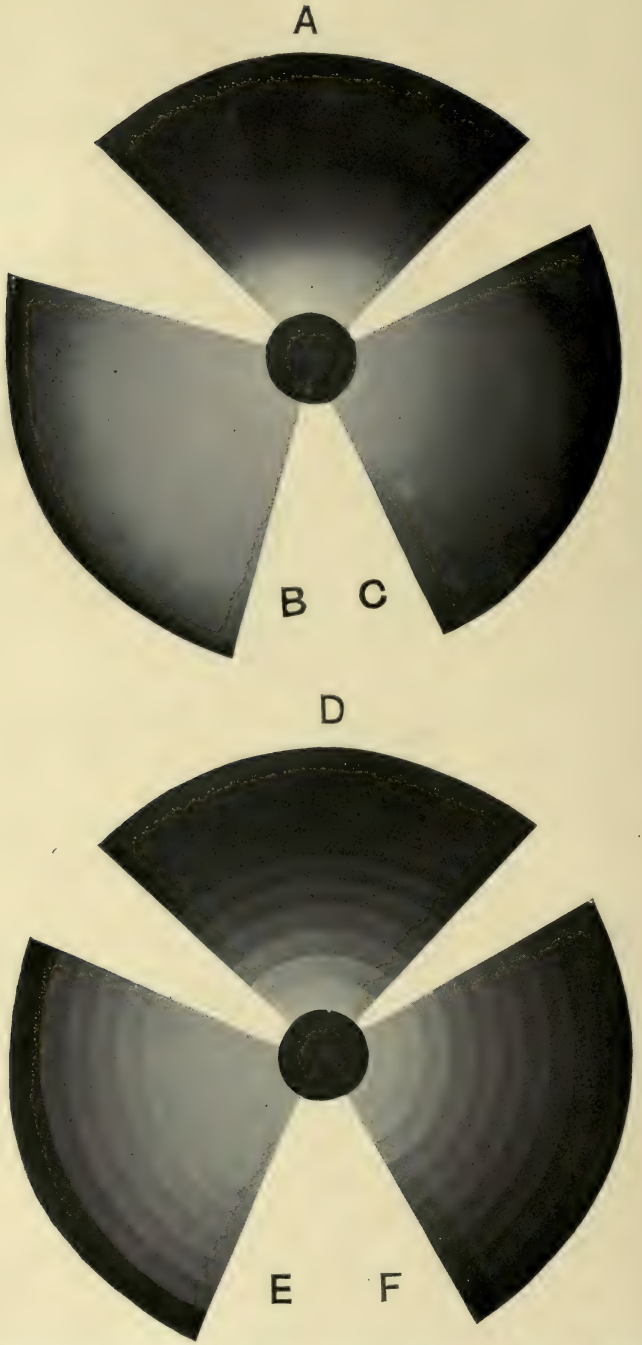
Mr. John D. Rockefeller has given $100,000 to Columbia University to endow the chair of psychology held by Professor Cattell.

The Congresses of the Paris Exposition include the first International Congress of Philosophy which will be held from the 2d to the 7th of August. There will be four sections: (1) general philosophy and metaphysics, (2) ethics, (3) logic, and (4) history of the sciences and history of philosophy.

M. Gabriel Tarde, well known for his contributions to psychology and sociology, has been elected to the chair of modern philosophy in the Collège de France.

Edwin Grant Dexter, A.M. (Brown), Ph.D. (Columbia), now of the State Normal College, Greeley, Colo., has been elected professor of pedagogy in the University of Illinois.

June E. Downey, A.M. (Chicago), has been appointed instructor in psychology at the University of Wyoming.



Lloyd Morgan on the Relation of Stimulus to Sensation in Visual Impressions.

VOL. VII. No. 3. MAY, 1900.

THE PSYCHOLOGICAL REVIEW.

ON THE RELATION OF STIMULUS TO SENSATION IN VISUAL IMPRESSIONS.

BY PROFESSOR C. LLOYD MORGAN, F.R.S.

The object of this investigation is briefly as follows: First, to produce what appears to the eye to be a smooth and even gradation from white into black through deepening shades of gray; secondly, to determine the proportional amounts of stimulus or physical excitation and to express them in a curve on the graphic method; and finally to ascertain the principles on which the amounts of stimulus and of sensation are related to each other. Colors have also been employed in the investigation with the primary object of obtaining a more extended series of data on which to found general principles. Secondary relations have however been brought out by their use.

The method adopted is based on that employed by Dr. Kirschmann.[1] Cardboard discs are prepared of a suitable size for use with a rotating table. Such a disc, say 374 mm. in diameter, is covered with black 'surface paper.' If a white sector, say of 30°, be affixed, and the disc be rapidly rotated, the black and white blend in visual sensation so as to give a uniform gray, the depth of which depends on the relative percentages of white and black. The blending is more perfect, however, if the same amount of white be added in three evenly-spaced sectors each of one-third the angular value, say 10°. It is clear that each sector increases in width from the center outwards; but the angle made by its boundaries is throughout the same. To increase or decrease the percentage of white we must increase or

[1] *American Journal Psychology*, Vol. VII., p. 386 (1897).

decrease the angle. And in all cases, at any distance from the center, the percentage is proportional to the angle subtended, or in the case of several sectors to the sum of the angles. This gives the method of introducing on to a disc any required percentage of white at any given distance from the center.

Let us suppose that our object is to ascertain whether equal increments of white stimulus, from some circle (a) near the circumference of the disc to some circle (f) near its center will afford evenly-graded shading from black, beyond the outer circle, to white within the inner circle. The black disc is marked out into three equal sectors by radii making angles of 120° with each other. One of such sectors is shown in Fig. 1. Arcs

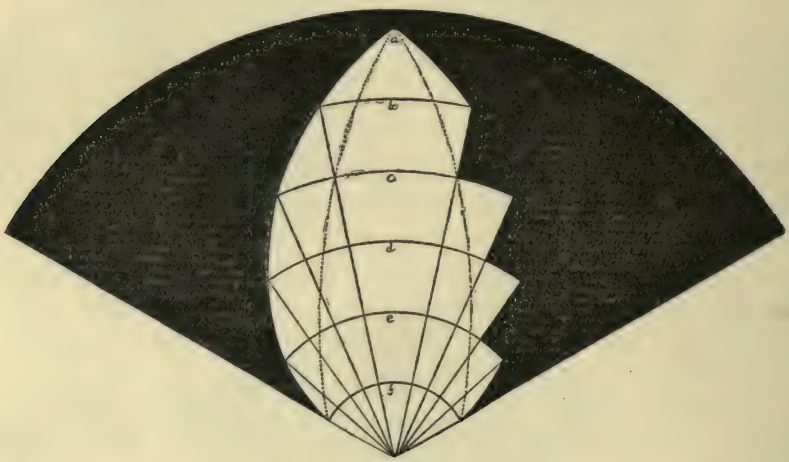


FIG. 1.

of circles are drawn at the required distances a and f; and four more arcs are drawn through points b, c, d, e on any radius, the distances ab, bc, ..., ef being equal. We have now marked out the portion of the disc with which we are to deal by six evenly spaced arcs, and we desire to introduce white in equal increments, thus:

at a,	0%
" b,	20
" c,	40
" d,	60
" e,	80
" f,	100

Bisecting the sector we measure off on each side of the bisecting radius the angles required to give these percentages. Since our measurements deal with one-sixth of the total amount of white to be introduced on the completed disc, and since the angular value of 1% on such a disc is $\frac{360^\circ}{100}$ or 3.6°, the value of 1% in each of our half sectors of 60° will be $\frac{3.6^\circ}{6}$ or .6°. Hence the angles we measure off are as follows:

at *b*,	12°	to give total of	20%
" *c*,	24°	" "	40
" *d*,	36°	" "	60
" *e*,	48°	" "	80
" *f*,	60°	" "	100

By sweeping a curve through points determined by these angular measurements an evenly graded increase of white from 0% to 100% is marked out as indicated by the boundary between black and white in Fig. 1. In practice the area thus limited is cut out (after tracing from a carefully prepared model) from white paper which is affixed to the black disc and if necessary accurately trimmed to give the true values of the percentages. All three sectors carry a similar white 'leaf.' By increasing the number of circles the number of measured points is increased and the curve may be drawn more accurately; and by checking the angular measurements, made with a protractor, by measurements of the chords, determined by calculation, yet further accuracy is secured.

On setting a disc so prepared in rapid rotation it is found *not* to give an evenly graded effect in sensation. Its inner central part is too white; the middle third much too light a gray to appear a good mean between white and black; while the outer third is again too light and ends off too sharply against the outer ring of black. Nor is the shading more satisfactory when similar areas are filled in with black on a white disc. Plate I., *B*, is from a photograph[1] of this disc in rotation. The central portion is too black and shades too rapidly into a light gray.

[1] The photographs from which the figures in the plates are reproduced were taken by Mr. Frank Holmes, of Clifton, Bristol. They serve to indicate, though they do not quite faithfully reproduce the effects of the eye.

In the paper above mentioned Dr. Kirschmann states that a logarithmic curve for stimulus affords with a rotating disc a means of exemplifying in a class experiment the truth of the Weber-Fechner formula, according to which the stimulus must increase in geometrical progression in order that sensation shall increase by evenly spaced increments. A disc prepared in strict accordance with the instructions given in Dr. Kirschmann's paper (save that black 'surface paper' was substituted for black velvet) gives an increase of white stimulus in geometrical progression from circumference to center. But it does not afford satisfactory grading in visual sensation. The inner third is too uniformly light, the peripheral portion too uniformly dark; and there is too sudden a transition from light to dark. There is no gentle and even passage from white into black through lighter and lighter shades of gray. Plate I., *A*, is from a photograph of this disc in rapid rotation.

A modification of this method of experiment serves to bring the investigation into closer touch with other psychophysical methods and with 'Masson's discs.' Instead of sweeping a continuous wave through, say, 10 determined points, white is added in step-like increments of the same proportional value. Thus to illustrate equal increments of stimulus the rotating disc gives ten rings in each of which there is a sudden increment of 10% of stimulus. This method of construction is illustrated to the right of Fig. 1. This disc is figured in Plate I., *E*, when it is in rapid rotation. Similarly a disc to illustrate a geometrical progression of stimulus gives also 10 rings which are figured in Plate I., *D*, when this disc is in rotation. In neither case do the rings give steps of equal value for sensation. By increasing the distance from which the rotating discs are viewed the steps become less marked to the eye until some of them disappear and seem to merge or shade into each other. But while some thus merge and lose their identity others remain easily distinguishable; which shows that they are not all of equal value for the eye. This method of experiment clearly brings the investigation into touch with that based on least perceivable difference. But a curious optical illusion somewhat distracts the judgment. Each ring is apt to appear double. Lying as it does between

a darker ring on the one side and a lighter ring on the other side it is peculiarly subject to the effects of contrast. Hence the part of it nearer the dark ring appears to be relatively lighter than the part near the lighter ring; and the physically uniform band differentiates in vision into a double band due to the effects of contrast.

Professor Kirschmann uses with his discs a small telescope for isolating small areas for comparison. Using tubes in a similar way I came to the conclusion that the shading of the disc as a whole affords a better means of judging of the grading than is obtained by examination of isolated areas in detail.

From many such observations as have been described, using both smooth and step-like increments of excitation, it appeared that neither an arithmetical nor a geometrical progression of stimulus affords an evenly graded series to the observer's eye.

The investigation here passed into the experimental stage. It was first necessary to ascertain what proportion of black and white would blend on a rotating disc so as to afford a sensation of 'mid-gray'—that is to say a shade which appears to the eye to be just mid-way between black and white. This was found approximately by means of discs with three concentric rings of about equal breadth, the inner black, the outer white, and an intervening ring with black and white sectors. The percentages for the papers affixed to the discs are from 25% to 28% of white, to from 75% to 72% of black, according to the brightness or other conditions of illumination. In a similar way it was found best to obtain a light gray of mean shade between this mid-gray and white. The proportions required are about 50% of white and black; and, to obtain a dark gray of mean shade between mid-gray and black, the requisite proportions are about 12.5% of white and 87.5% of black. Considerable difficulty was, however, experienced in judging a given ring to be an arithmetical mean strictly half way between those of higher and darker shades; and it was found that continuous curves yielded better results. After some experience the eye detects even slight departures from smooth and even grading in the sensory series.

It is here desirable to give in some detail the experimental results obtained with eight discs. They will serve to show the

way in which the requisite percentages of white stimulus on a black background were determined and errors eliminated. The observations were made at a constant distance of 10 feet from a mirror and, so far as possible, under illumination of constant intensity. Measurements were made at 11 stages, from periphery to center, numbered in the first column of the accompanying table. The full number of 20 stages would give equal sensation, increments of 5%; but sufficient accuracy was secured by measurements at intervals of 10% increments, except at each end of the graded series where the 10% is divided into two stages of 5% each. The discs are lettered A–H. The percentages given under A represent those which had already been reached by earlier observations and which gave fairly good grading but not altogether satisfactory at either end of the graded scale. B C D were constructed from curves varying in different ways around that which represents the percentages of A. E combines the net results of observations on A–D; F and G introduce slight + and − modifications of E; and finally H gives the net result of observations on the whole series. In each case 27% (a value determined by the conditions of illumination) was taken as a datum for the mid-gray. Appended to the table are the notes made at the time as a guide to the further experiments embodied in subsequently-made discs. The inner lines of Fig. 1 give the boundaries of the leaf-shaped area of white on a black ground in each of the three equal sectors of the disc lettered A in the table.

TABLE I.

Stage.	Sensat.	A	B	C	D	E	F	G	H
20	100	100	100	100	100	100	100	100	100
19	95	85.	87.6	90.2	82.4	89.5	89.	90.	89.
18	90	73.	76.9	80.8	69.1	79.	78.	80.	79.
16	80	56.5	59.2	61.9	53.8	62.	60.	64.	62.
14	70	44.5	45.6	46.7	43.4	47.	46.	48.	48.
12	60	35.2	35.2	35.2	35.2	35.2	34.3	35.8	35.8
10	50	27.	27.	27.	27.	27.	27.	27.	27.
8	40	20.2	20.9	21.6	19.5	19.5	20.	19.	19.5
6	30	15.	16.2	17.4	13.8	13.	13.6	12.4	13.
4	20	10.	12.1	14.2	7.9	7.9	8.5	7.3	7.9
2	10	5.2	6.8	8.4	3.6	3.5	4.	3.	3.5
1	5	2.7	3.6	4.5	1.8	1.8	2.	1.6	1.8
0	0	0	0	0	0	0	0	0	0

NOTES.

A. Stages 1–6 too white; 14–19 too dark; mid-region satisfactory.

B. " 1–8 much too white; 14–19 better than A.

C. " 1–8 very much too white; 14–19 better than A or B.

D. " 1–6 good; 14–19 much too dark. Make new disc (E) combining 1–6 of D, 14–19 of C, and mean of intermediate stages.

E. Good result; weakest 12–14, too dark. Try two new discs (F and G) with maximum + or — modifications to amount of 2 %.

F. Not so good as E as a whole; 18–19 good; 12–14 worse rather than better. 1–8 rather too white.

G. Not so good as E on the whole; 12–16 better than E or F; 18–19 much the same but not quite so good as E. Take results of E modifying 12–14 to accord with G.

H. Satisfactory result. A little too dark near 12. Accept as basis for theory.

From these results it will be seen that slight errors are more easily detected where the percentages of white are small than where they are relatively large. A study of the percentages will further show that whereas the first 5% of sensation is produced by the introduction of about 2% of white stimulus, the last 5% (from 95% to 100%) is produced by the introduction of 11% of white. Hence a small absolute addition of white to low percentages will be noticeable, whereas a much larger addition to high percentages will remain unnoticed.

Colors and shades were next dealt with, and experiments made with several pairs, such as red and black, blue and black, red and white, red and blue, purple and emerald green. The object here was to get a sufficiently wide series of data on which to base a discussion of the relation between stimulus and sensation. It was found that each pair gave a differing series of percentages. It will suffice to present the final results obtained with red on black, and blue on black. They were reached by methods similar to those already illustrated in sufficient detail. It should be stated that the results embodied in the accompanying Table (II.) were attained by an extended series of experiments and observations analogous to those given in Table I.

So far the results obtained are purely empirical. They were plotted in curves on the graphic method with a view to determining if possible the law which underlies them.

Before proceeding to a consideration of this law a criticism which may suggest itself must be met. It may be said that the

TABLE II.

Stage.	Sensation.	White on Black.	Red on Black.	Blue on Black.
20	100	100	100	100
19	95	89.	91.	94.5
18	90	79.	83.5	89.1
16	80	62.	69.	78.4
14	70	48.	55.5	67.9
12	60	35.8	44.5	57.6
10	50	27.	35.	47.5
8	40	19.5	25.5	37.6
6	30	13.	18.	27.9
4	20	7.9	12.	18.4
2	10	3.5	6.	9.1
1	5	1.8	3.	4.5
0	0	0	0	0

'black' and 'white' employed are very far from being absolutely black and white respectively; and that the colors used—surface paper and wall papers—are very far from being specifically pure. This is perfectly true. What is sought, however, is to obtain even grading between a sensation a on the one hand and a sensation x on the other hand; or, since neither visual sensation is pure, let us say a complex sensory effect a, d, g, on the one hand and another sensory effect s, v, z, on the other hand; and this may be attained no matter how complex either term may be. Experiment showed, for example, that a grading could as readily be obtained between 'black' and light gray as between 'black' and 'white'; the mid-point between the two being obtained by using 30% of the gray selected and 70% of 'black.' And so far as the colors are concerned the results appear to be due rather to the relative intensities of stimulation or excitation of the retina than to the effects of color as such. In any case there is no pretence that this investigation deals with pure colors or shades, though it may lead up to a further investigation of spectral colors.

Another point which calls for notice is the illumination. This is a matter of considerable importance. In a bright light the contrast between colors or shades of different intensities is heightened. A black and white disc which shows good shading for a medium illumination fails to grade smoothly in a strong light. But though the percentages and curve for any pair of

colors or shades varies with the conditions of lighting, the principle which it illustrates remains the same.

Proceeding now to a consideration of the law which appears to express the relation between stimulus and sensation it will be well to begin with a discussion of the curves for 'white on black,' 'red on black' and 'blue on black' which very closely accord with those which are plotted in Fig. 2. And the first

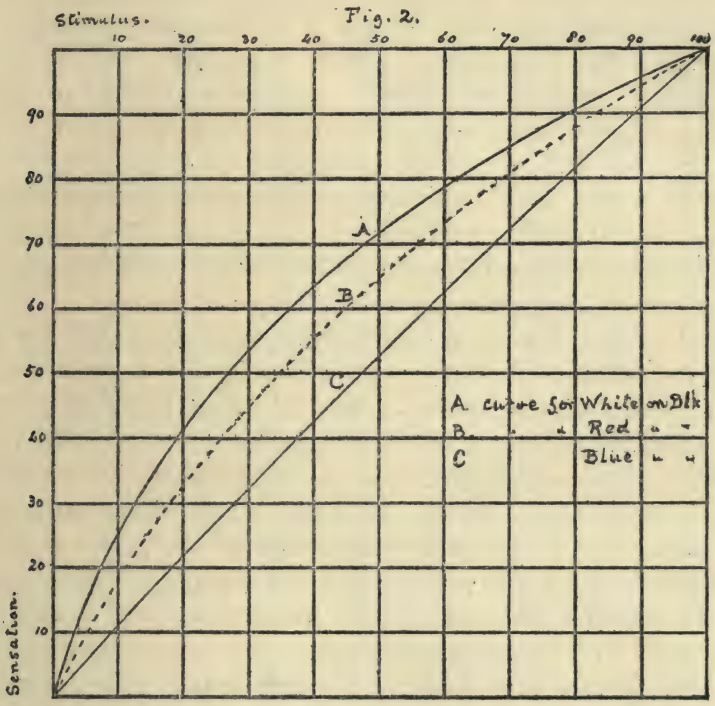


Fig. 2.

point to notice is that neither of these curves is throughout its whole extent logarithmic as it should be if the Weber-Fechner formula holds good. It is true that curves expressing geometric progression of stimulus can be found in each case nearly to fit that obtained by observation *in limited portions of its extent.* But the curves of Fig. 2 are so homogeneous that they seem to indicate some simple law applicable to all, and in each case throughout the whole range. And this law is certainly not that expression by the Weber-Fechner formula. It is well known how-

ever that this formula fails in cases of low and of high stimulation—a defect which is somewhat lamely explained by saying that it only holds good for the *optimum* region of sensation. It is generally admitted to be but an approximation to the true interpretation of the facts. A law which shall apply to the whole range of normal sensation is still a psycho-physical desideratum.

It will be remembered that the Weber-Fechner formula generally accepted runs as follows: In order to produce a series of just distinguishable sensations, giving an arithmetical progression of sensation, the determining stimuli must afford excitations in geometrical progression. But in the curves plotted in Fig. 2 the stimuli required to produce the sensation series 5%, 10%, 15% etc. are *not* in geometrical progression. This being so the question arises: Do they form an orderly series conforming to some simple law of progression? They do, and the law may be thus stated: *Equal increments of sensation are produced by increments of excitation in geometrical progression.*

Let us fix our attention on curve A in Fig. 2, which represents the relation of stimulus to sensation for a graded series from 'white' through gray to 'black.' Now experiment shows that 27% of 'white' stimulus affords the sensation midway between 'white' and 'black'—in other words it corresponds to 59% of sensation. Let us take this single observed fact as the *datum* for a calculation based on the suggested law, and then see how far the percentages that result from this calculation accord with those obtained in the experimental work above described. Dividing the whole range into 10 equal sensation-increments of 10% each we have to ascertain the related stimulus-increments. It is clear that the sum of the first five stimulus increments gives the observed number 27% while the sum of the completed series of 10 stimulus-increments gives the total of 100%. Hence

$$100 = \frac{a(r^{10} - 1)}{r - 1} \qquad (1)$$

$$27 = \frac{a(r^{5} - 1)}{r - 1} \qquad (2)$$

where a is the first step and r is the factor to give a geometrical series; from which the value of a ($= 3.49$) and

that of r (= 1.22) are readily obtained,[1] and these being known the value of each of the ten terms which make up the complete series may be calculated. The following Table (III.) gives the increments of sensation and their sums, the increments of stimulus and their sums, and (for comparison) the percentages obtained by experiment.

TABLE III.

WHITE ON BLACK.

Stage.	Sensation.		Stimulus.		Observed Percentage of Stimulus.
	Increments.	Sums.	Increments.	Sums.	
20	10	100	20.90	100	100
18	10	90	17.13	79.10	79.
16	10	80	14.03	61.97	62.
14	10	70	11.51	47.94	48.
12	10	60	9.43	36.43	35.8
10	10	50	7.73	27.	27.
8	10	40	6.33	19.27	19.5
6	10	30	5.20	12.94	13.
4	10	20	4.25	7.74	7.9
2	10	10	3.49	3.49	3.5
0	0	0	0	0	0

In Fig. 3 the curve for 'white on black' is plotted on the graphic method and the manner of its construction with step-like increments in geometrical progression shown.[2]

$$^{1}\ 100 = \frac{a(r^{10} - 1)}{r - 1} \qquad (1)$$

$$27 = \frac{a(r^{5} - 1)}{r - 1} \qquad (2)$$

Hence by division

$$\frac{100}{27} = \frac{r^{10} - 1}{r^{5} - 1} = r^{5} + 1$$

$$\therefore r^{5} = \frac{100}{27} - 1 = \frac{73}{27}$$

$$\therefore 5 \log r = \log 73 - \log 27.$$

Whence $r = 1.22009$

Substitution in (2) gives $a = 3.488$.

The series of increments for 10 stages will be as under:

Stage	1.	2.	3.	.....	9.	10
	a.	ar.	ar^2.	.....	ar^8.	ar^9.

[2] The following note is kindly furnished by my colleague, Professor F. R. Barrell, to whom I am much indebted for valuable criticisms and suggestions.

Denote intensity of sensation by x,
" " " stimulus by y,
y must be some function of x, say $f(x)$.
Let three equal increments h be given successively to x and let $\kappa_1, \kappa_2, \kappa_3$, be the corresponding increments of y

$$\therefore y = f(x)$$
$$y + \kappa_1 = f(x+h)$$
$$= f(x) + f'(x)h + f''(+)\frac{h^2}{1.2} + f'''(x)\frac{h^3}{1.2.3} + \cdots$$
$$y + \kappa_1 + \kappa_2 = f(x+2h) \qquad \text{[by Taylor's theorem]}$$
$$= f(x) + f'(x).\,2h + f''(x)\frac{4h^2}{1.2} + f'''(x)\frac{8h^3}{1.2.3} + \cdots$$
$$y + \kappa_1 + \kappa_2 + \kappa_3 = f(x+3h)$$
$$= f(x) + f'(x).\,3h + f''(x)\frac{9h^2}{1.2} + f'''(x)\frac{27h^3}{1.2.3} + \cdots$$

$\therefore$ By subtraction

$$\kappa_1 = h\{f'(x) + \tfrac{1}{2}f''(x)h + \tfrac{1}{6}f'''(x)h^2\}$$
$$\kappa_2 = h\{f'(x) + \tfrac{3}{2}f''(x)h + \tfrac{7}{6}f'''(x)h^2\}$$
$$\kappa_3 = h\{f'(x) + \tfrac{5}{2}f''(x)h + \tfrac{19}{6}f'''(x)h^2\}$$

Now the hypothesis is that $\kappa_1\kappa_2\kappa_3$ are in G. P.

$\therefore \kappa_2^2 = \kappa_1\kappa_3$.

Substitute the above values for $\kappa_1, \kappa_2, \kappa_3$, we find that the absolute terms and the coefficients of h are identical on both sides of the equation; the coefficients of h^2 give

$$\tfrac{1}{6}ff''' + \tfrac{5}{4}f'^2 + \tfrac{19}{6}f'f''' = \tfrac{7}{3}f'f''' + \tfrac{9}{4}f'^2$$
$$\therefore f'f''' = f''^2$$
$$i.\,e.,\ \frac{dy}{dx}\frac{d^3y}{dx^3} = \left(\frac{d^2y}{dx^2}\right)^2.$$

The solution of this differential equation is

$$y = Ae^{Bx} + C.$$

But sensation $x = 0$ when stimulus $y = 0$ $\Big\}$ $\therefore C = -A$

$$\therefore y = A(e^{Bx} - 1) = A(10^{bx} - 1) = A(\lambda - 1)$$

where A, B, b, λ are constants whose values are found by making curves pass through the point (100, 100) and any other one point given by experiment.

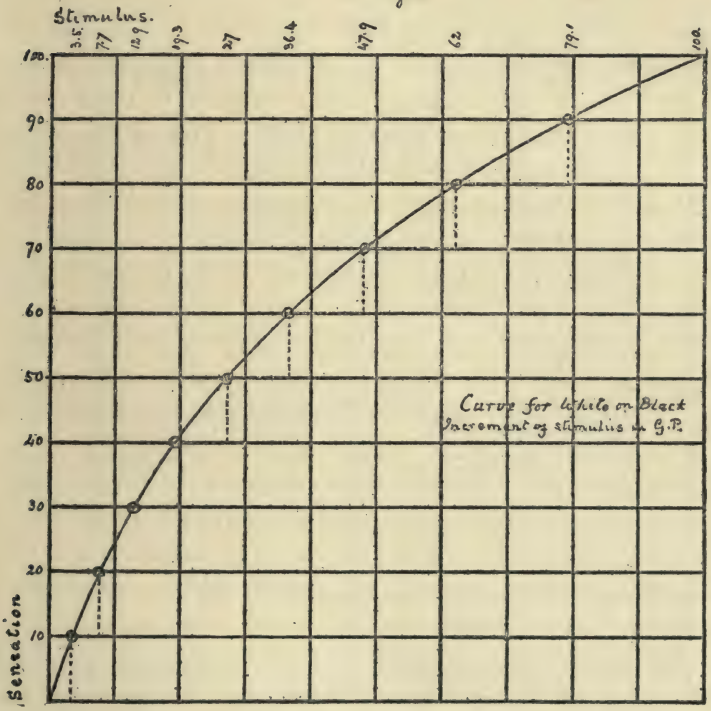
[In the body of the paper sensation is represented by y and stimulus by x $\therefore$ x and y should be interchanged in above equations.]

For the curve 'white on black' $A = 15.85$
$b = .0086392$
for the curve 'red on black' $A = 40.835$
$b = .005377.$

F. R. B.

A disc prepared in accordance with the percentages given by the theory affords good and even shading (Plate I., *C*). Another disc in which the increments in 10 stages are introduced step-fashion gives a series of rings which on the whole grade well, though the effects of contrast have to introduce a disturbing element. When such a disc is viewed from progressively increasing distances, all the rings merge into a continuum together.[1] Some of the rings do not disappear earlier than

Fig. 3.



others as in the case to which allusion has previously been made. Plate I., *F*, is from a photograph of this disc when it is in rapid rotation.

It will be noticed that in the theoretical series,[2] as in the

[1] This does not hold good of the accompanying photographic reproduction, which is so far inaccurate.

[2] Alike in the experimental series and in the theoretical interpretation of the results 27% of white is accepted for mid-gray. Under the conditions above

closely similar series of percentages reached by experiment, a much larger absolute addition of 'white' is required to give say 10% increment of sensation at one end of the scale than is required at the other end of the scale. It is well known that a smaller amount of white is just perceivable on a black ground, than of black on a white ground. This may be illustrated by means of rotating discs. It will, for example, be found that a gray ring produced by 5% of white on a black ground is more marked than a gray ring produced by 10% of black on a white ground, notwithstanding the fact that the amount of white in the more obvious ring is but half that of black in the less readily distinguishable gray circle. More careful experiments showed that, whereas the difference between 99% and 100% of white (that is, 1% of black on a white ground) is just perceptible, from .3% to .4% of white was just perceivable on a black disc, the exact amount varying with the brightness of the illumination. Now the increment of white on black which corresponds (in accordance with the law here suggested) to 1% of black on white is .38%. Too much stress should not be laid on the accordance of the two results; but it may be given for what it is worth.

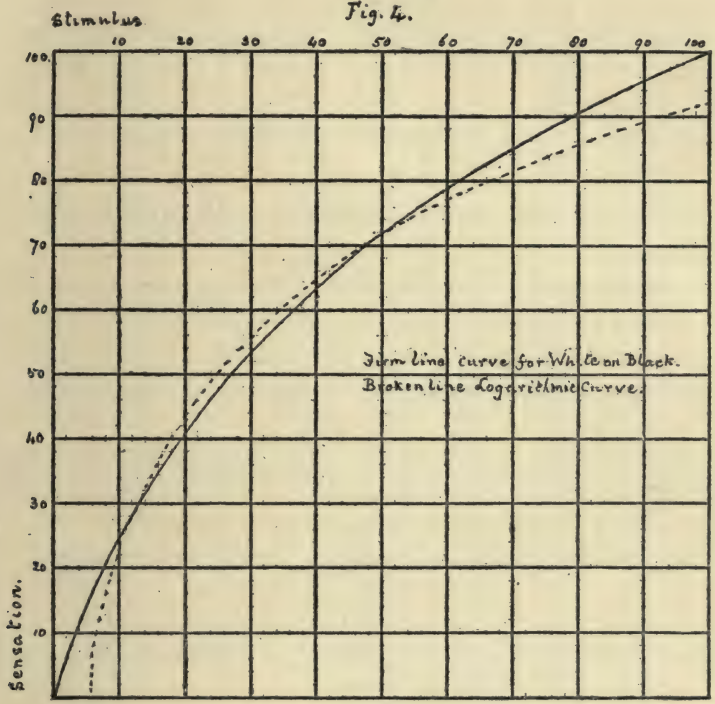
It has already been stated that the curve founded on observation, on which the theoretical curve is based, differs from a logarithmic curve. This may be illustrated by Fig. 4. The firm line gives the theoretical curve based on the principle of increments of stimulus in geometrical progression. The broken line shows the logarithmic curve which passes through the percentages at stages 6 and 14. Although this is one of the best logarithmic curves which can be found for purposes of com-

mentioned, with a medium illumination this to my eye gives good shading. With a brighter light, or at a smaller distance, the percentage of white has to be diminished. For though the eye is capable of some amount of adaptation to the amount of light received, adaptation effected through changes in the size of the pupil, there remain outstanding differences. For the distance at which the photographs for the accompanying plate were taken the percentage to give mid-gray was reduced to 25%. Some of my friends consider my mid-gray too light; a few regard it as too dark. It seems that the personal equation, and the sensitiveness of the individual retina must be taken into consideration. I would therefore lay no special stress on the percentage which for my eye gives mid-gray; that on which I do lay stress is that, when this datum is determined, for any given individual under given circumstances, the relative percentages have values in accordance with the law of progression above set forth.

parison, it will be seen that the divergence is far in excess of any probable errors of observation.

Passing now to the curve for 'red on black' (which is shown graphically in *B*, Fig. 2) the following Table (IV.) gives the application of the law in a manner similar to that in the previous Table (III.) for 'white on black.'

Fig. 4.



Since the excitation effect of the red employed is less than that of 'white' a larger amount of stimulus (viz, 35% as compared with 27%) is required to give a sensation midway between red and black. The curve, therefore, passes through this point instead of through 27%. The value of *a* (the first of 10 steps) is here 5.382 and that of *r* 1.1318; but the law still holds good. Equal increments of sensation are produced by increments of excitation in geometrical progression.

It is unnecessary to give details with regard to the curve for dark blue on black in the experimental work for which I have

TABLE IV.

RED ON BLACK.

Stage.	Sensation.		Stimulus.		Observed Percentage of Stimulus.
	Increments.	Sums.	Increments.	Sums.	
20	10	100	16.40	100	100
18	10	90	14.49	83.60	83.5
16	10	80	12.80	69.11	69.
14	10	70	11.31	56.31	55.5
12	10	60	10.	45.	44.5
10	10	50	8.83	35.	35.
8	10	40	7.80	26.17	25.5
6	10	30	6.90	18.37	18.
4	10	20	6.09	11.47	12.
2	10	10	5.38	5.38	6.
0	0	0	0	0	0

been assisted by Miss Alice Worsley. It will suffice to give the percentages of blue stimulus ($a = 9.124 : r = 1.02022$).

It will be observed that the progression is nearly arithmetical. But a strict arithmetical progression does not give satisfactory results. Even when 48.5% instead of 47.5% was taken for the mid-point, the shading was distinctly less satisfactory.

BLUE ON BLACK.

Stage.	Increments.	Percentage of Stimulus.
20	10.926	100
18	10.708	89.074
16	10.495	78.366
14	10.287	67.871
12	10.084	57.584
10	9.884	47.500
8	9.688	37.616
6	9.496	27.928
4	9.308	18.432
2	9.124	9.124
0	0	0

In the cases considered in this paper we have positive stimuli —excitations due to the white, red or blue on a background of black, which may be regarded as incapable of affording any appreciable amount of positive stimulation to the retina.[1] When we pass, however, to cases of shading where two positive stimuli are concerned (such as white into red, red into blue, orange into red) we obtain curves which differ in type from those given

[1] I do not think that the *Eigenlicht* modifies the results in any appreciable degree.

above. I propose to deal with these in a subsequent paper. The experimental results and their discussion are well advanced; and the principle involved appears to support the conclusions already reached. It must here suffice to say that the interaction of different stimuli and the effects of contrast render the problem more complex and the curves less simple.

Leaving on one side therefore the interpretation of the results of observation on the mutual shading of colors, or of a color and white, which undoubtedly indicate some more or less complex mode of interaction, attention may be recalled, in conclusion, to that which it was the main purpose of this investigation to determine—the relation of sensation to stimulus in the relatively simpler cases of white, red and blue on a black, and probably negative, background. Observation seems to show that for the phenomena under consideration the Weber-Fechner formula cannot be accepted as it stands in the text-books. The law that the observations here recorded seem to indicate is that *equal increments* of sensation are produced by *increments of stimulus in geometrical progression*. Still, as we have seen, the Weber-Fechner formula is approximately applicable to limited portions of the curves in Fig. 2, though in none of the cases, even approximately, to its whole extent. But since the majority of observations which accord best with the Weber-Fechner formula have been made within a *limited* optimum range of sensation, they would probably not be widely divergent from the requirements of the formula which results from my research. Hering's pupils came to the conclusion that, near the threshold of visual sensation the stimulus increases in arithmetical progression. Now a glance at the curves of Fig. 2 will show that the progression near the threshold differs so little from arithmetical that it would be difficult by experiments on least observable difference to determine the departure therefrom. These observations also may therefore be held to be in substantial accord with the psycho-physical formula which is here suggested.

No attempt can be made in this paper to correlate the results obtained with those reached by previous observers in the field of vision.

A NEW EXPLANATION OF WEBER'S LAW.

BY THE LATE DR. LEON M. SOLOMONS.

The theory of the threshold which I wish to present here was suggested during some experiments upon the perception of sudden pressure changes, and is perhaps best approached from that point of view. If to a pressure, s, we suddenly add an increment, ds, without removing the original pressure, the change is perceived, not by a comparison of the old pressure, s, with the new pressure, $s + ds$; but directly as such. The act of comparison upon which Wundt founds his explanation of Weber's Law is reduced to a minimum at least, if it is not entirely lacking. Experiments of this kind therefore furnish a means of deciding between the two rival theories, or at least of deciding against Wundt's; for the results seem to follow Weber's Law, notwithstanding the fact that there is no comparison of two separate stimuli in the process of perceiving the added pressure. The experiments, however, suggest a view of the threshold entirely different from either of the theories. For the objectively constant pressure is not sensed as constant at all, but seems to fluctuate; undergoing those well known changes which, for lack of better name, are called fluctuations of attention. Now the whole difficulty in perceiving the added pressure, or the pressure change, lies in the difficulty of distinguishing a change in the objective pressure from these apparent changes due to fluctuations of attention. For the added pressure to be perceived, and identified with certainty, it must be greater in magnitude than these normal variations of a constant pressure. It seems possible that we have here the true theory of all threshold phenomena, or rather the basis of a theory. The explanation is to be sought neither in the nature of comparison, nor in the quantitative relations between stimulus and brain reaction, but in the well known fact of variability of brain activity under identical stimuli. The threshold simply measures the range of

this variability. *Two stimuli must differ by more than the range of this variability for their difference to be perceived*, for the same reason that it is necessary to have a difference between two physical quantities you wish to measure greater than the probable error of your measurement method, if you wish to detect the difference with certainty. A difference between two quantities less than the threshold is overshadowed by the variations in each. A brief development of the theory follows.

Assumptions.—The intensity of any brain process depends both upon the intensity of the stimulus calling it forth and upon the condition of the reacting center. This latter will in turn depend upon a variety of bodily and mental conditions which are constantly changing. If we represent by the letter I the resultant of all these conditions, then we have the formula $S = Is$, that is the intensity of the resulting process, usually a sensation, varies as the product of the irritability of the reacting center and the external stimulus; the constant of the ratio being omitted for the sake of simplicity. When I remains constant S is proportional to s, when s is constant S is proportional to I. The law is analogous to innumerable relations with which the other sciences make us familiar.

The Threshold in General.—Since I is subject to constant variation the sensation resulting from any stimulus varies likewise. Accordingly, if we are affected in succession by two stimuli of the same intensity sometimes one, sometimes the other will seem to be the greater, depending upon the value of I at the moment. In general one will seem to be greater as often as the other, so that if in fact there is a very slight difference between the stimuli the judgment will be correct as often as incorrect. As this difference increases, however, the proportion of correct judgments will increase; for as the differences in the value of I are of all grades, it will often happen that the actual difference in the value of s will counteract the difference in the value of I and a stimulus be correctly judged as greater than the preceding, though the value of I be less. For any given difference between the stimuli there will be a corresponding percentage of correct judgments, the percentage depending upon the extent and distribution of the variations in

. The probability of a given difference being perceived is the probability of the difference between the two values of I being less than the difference between the stimuli. One hundred per cent. of correct judgments will only be reached when the difference between the stimuli is more than twice the greatest variation of I.

Weber's Law.—That the least perceptible difference should be a constant proportion of the stimulus follows at once from the formula. The effect upon S of a variation in the value of I is obviously proportional to s. Weber's Law has in fact innumerable illustrations in the physical world. The variability of a quantity which depends upon a variety of other quantities is always a percentage variability. To take a few of many possible illustrations, the volume of a quantity of gas depends upon its temperature and the volume it would have at some standard temperature. The volume changes then produced by a change in temperature will be proportional to the volume it would have at the standard temperature, pressure remaining constant. The strength of the current developed by a dynamo depends both upon the speed of revolution and the strength of the field magnets; if either remain constant it is proportional to the other. Now the change produced in the strength of the current by a change in the intensity of the magnetic field is proportional to the speed at which the armature is revolving, and conversely the change produced in the current by a variation in the speed is proportional to the intensity of the field. If the speed is subject to constant fluctuations—as it might be if there was no fly wheel—and one wished to detect a change in the intensity of the field by means of the change in the current, it is clear that the change would have to be greater than the changes due to the variations in speed alone; and as these variations due to change in speed are proportional to the intensity of the field, the change in the field, to be detected in this way, would have to be a constant fraction of itself. It is a perfect analogy to Weber's Law, where the change in the stimulus, in order to be detected as a change in the joint product of stimulus and irritability, must produce a change in this joint product greater than that produced by the variations in irritability alone;

and since the changes in irritability produce changes in the product proportional to the value of the stimulus s, the changes in s must be proportional to itself also. Mathematically the relation is expressed as follows: The variations of S due to I are given by the equation $dS = dI\,s$, while those due to the actual change of s by $dS = I\,ds$. For the threshold these values must be equal, which gives $I\,ds = dI\,s$, whence $ds = s\,dI/I$, or the threshold for perceptions of difference is proportional to the stimulus s, the factor of proportionality being the ratio of the variations of I to the value of I—that is the percentage variations of I.

The Value of the Threshold Ratio.—The last formula gives immediately the significance of the threshold ratio. It measures not the delicacy of a sense, but its constancy; it is the variation ratio of the irritability. We should expect it to be smallest, therefore, not in those senses that are the most delicate necessarily, but in those where constancy of action is important. It is very high in sound because sounds seldom do keep the same intensity for long, and the practical recognition of objects does not demand constancy of functioning here. Yet the ear is a very delicate mechanism, and where constancy is important for the recognition of objects, as in the detection of pitch differences, the threshold is low—if we may speak of a threshold here. On the other hand with the eye, which is normally acted upon by stimuli which remain for a considerable length of time, we should expect greater constancy, especially where the distinguishing of objects, as in the shades of gray, depends upon it. Here accordingly we find a very low threshold.

The Constant Error.—We have hitherto supposed that I and s vary independently, and have not considered the various factors that make up I. It is obvious, however, that this will not always be the case. Among the factors which determine I must be reckoned the character of the previous stimuli of the part of the brain in question, and the length of time which has elapsed since. In so far as I depends upon such factors its variations will not be distributed according to the law of chance, so that one stimulus is treated the same as another, but it is possible for the second stimulus, for example, to be always accompanied by a slightly greater value of I. We can not

explain any particular constant error until we know more about the factors which make up I, but it is clear that among these factors there will probably be some that depend upon the position of the stimulus, and thus to that extent the law of chance will no longer hold for the distribution of the variations, and we have a constant error.

Variations from Weber's Law.—The same considerations serve to explain the variations from Weber's Law. According to our formula the threshold varied as the product of the stimulus and the percentage variability of the irritability. Weber's Law should hold, therefore, as long as the variations of the irritability remain the same. The threshold ratio will vary as soon as new factors are introduced into I. This means not only that conditions must be the same as to fatigue, attention, etc., but that the stimulus itself does not affect the irritability of the brain. If new factors are introduced into I by the stimulus the law no longer holds. It is not surprising, therefore, that the deviations should be especially marked at the extremes of intensity. The fact of adaptation of end organ to stimulus is so universal that we should be surprised if the stimulus was not a factor in the irritability. The general fact of Weber's Law being only approximate therefore is not surprising. The law is explained upon the assumption that s and I are independent variables, while in fact I is to some extent a function of s. But now as to the character of the variations. The threshold being dependent upon the variability of I is not necessarily either raised or lowered by a change in the value of I (unless the change is so rapid as to give different average values of I for the two stimuli compared). The question is, How we may expect the new factors to affect the *constancy* of I?

It will obviously depend both upon the variability of the new factor and its actual effect upon the value of the irritability. There is one case, however, that is worth considering, because the explanation it gives of a common phenomenon is peculiar to this theory of the threshold. If we consider any brain process as made up of a number of constituent processes in separate, though similar, brain elements, then the intensity of the entire process will be the sum of the whole, and the irritability, as we have

defined it, the average irritability of the separate elements. Now there will presumably be, among the elements entering into this irritability, some which affect the whole group at once and some which act separately upon the individual cells. Now so far as these latter causes are concerned, their result will be that sometimes one cell, sometimes another, will be at its maximum irritability; and if a very large number of them are stimulated at once these variations will neutralize each other, and the resultant variability of the whole group be reduced to that due to causes which affect all the members of the group in the same way at the same time. Other things being equal therefore the variability will be at a minimum when the largest possible number of cells are concerned in the process, and at a maximum when the smallest possible number are concerned. Therefore we should expect that the threshold ratio should be very high for feeble stimuli, and decrease rapidly at first, more slowly later, as the stimulus increases in intensity, and therefore spreads over a larger brain area.

An interesting corollary to this explanation of the variation of the threshold ratio is furnished by considering what would happen if we could by any means get such control of these individual sources of variation as to make them vary synchronously. Clearly the effect would be to increase greatly the variability of sensations from stimuli of ordinary intensity. Now just this we seem to have in the phenomena of rhythmical accent. The regularity of the stimuli seems to bring the sources of variation into synchronism with each other, and thus lead to regularly recurring periods of maximum irritability. Part of this at least is certainly easy to understand. Among the factors which determine the irritability of a cell must be reckoned the length of time since the last stimulus. The regularity of the stimuli in the case of the rhythm must tend to bring this element gradually under control, with the resulting phenomena of marked differences in intensity.

Conclusion.—The advantages claimed for this theory are that it treats the whole of threshold phenomena together, and finds the explanation of Weber's Law in the facts which account for the existence of the threshold. It should be noticed that not

only is this variability of the sensation resulting from a particular stimulus a very probable fact, and one which we can almost claim to observe directly; but *has to be assumed in any case by all theories* of Weber's Law, in order to explain the fact that the threshold is not a definite, sharply defined quantity—to explain, in fact, that variability of judgment upon which the error method and the method of right and wrong cases are based. *But once grant this variability and nothing more is necessary; all the phenomena connected with thresholds follow easily from this.* Both the reigning theories offer simply an *interpretation* of Weber's Law, which does not connect it with other threshold phenomena, nor with any other well known psychological or physiological phenomena.

The hypotheses upon the basis of which the law is explained are supported by little more than the law itself. They neither explain the unknown by the known, nor by an hypothesis which has value elsewhere. Thus no simplification is effected, nor is the law brought into connection with any other facts. In this respect Müller's theory is perhaps a little ahead of Wundt's, but not very much; for the special relation demanded between the proportion of overflow and effective stimulus is purely gratuitous. The present theory seems to enable us to deduce Weber's Law from the known fact of variability of brain process with constant stimulus, and thus to bring the law into connection not only with other threshold phenomena, but with such well known phenomena as fluctuations of attention, rhythmical accent, etc. I have said the known variability of brain process—this is perhaps an exaggeration. But the variability of sensation from a constant stimulus certainly comes very near ranking as a simple fact of observation, and at least it is an hypothesis that we are forced to by *many different* facts; and not one gratuitously invented for this special purpose.

ELEMENTS OF PSYCHOLOGICAL THEORY OF MELODY.

BY MAX MEYER, PH.D.,

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In the following pages I intend simply to describe musical facts by laws, not to explain their existence. I have not been fortunate enough to discover the cause of consonance, and therefore readers who are interested in explanations only may lay aside this paper at once.

There are plenty of books on musical theory, written by professional musicians, physicists, physiologists, psychologists and philosophers. But when one has studied them throughout in order to find *psychological* laws one must confess finally that he has not found any. The only investigator in psychological literature who hit the right path is Th. Lipps. However, after having followed this path a short distance, he turned into the common path, more conspicuous because more frequented, which crosses the right path many times, but does not lead, as that one does, to the top of the mountain, but after devious windings leads back again to the starting point. In all the bulky books of musicians on musical theory I have sought in vain for scientific laws. No doubt, there are laws in those books, but they are not scientific, as they are true only in a limited number of special cases and contradict the rest.

The wrong path, much frequented, which inevitably leads back to the starting point, is the adoption of the theory that the basis of all music is the so-called diatonic scale, represented by the numbers 24, 27, 30, 32, 36, 40, 45, 48.

From this theory arise all the errors which prohibit the development of a scientific theory of music. The adoption of that theory prevented Lipps from carrying his investigations farther. The same mistake prevented Gurney (in his very good book 'The Power of Sound') from solving the problem, which he

clearly shows as unsolved, and led him to the conclusion that it was 'hopeless to think of penetrating music in detail,' because he had found no way himself. Still I do not understand why he undertook to write a book of 600 pages on music while believing that it was "hopeless to think of

> Untwisting all the chains that tie
> The hidden soul of Harmony."

The only right conclusion is that the way he followed was the wrong one. Unable to explain the real existence in music of those notes which have no theoretic existence in the diatonic scale, he attempted to explain them in a manner that has become quite common in recent years, assuming that the place of such notes in a musical form, relationship being in abeyance, was wholly due to close propinquity.

The consequence of this theory would be that the tones corresponding to the numbers 10, 11, 12, 11, 10 would result in the formation of a melody, 10 and 12 being connected by relationship, 11 with either 10 and 12 by propinquity. Should the propinquity be not close enough, we might take the numbers 30, 31, 36, 31, 30. To listen only once to such a tune is quite sufficient to be convinced of the perverseness of that theory. No melody can be formed by the help of propinquity. *No tone that appears in music is really without relationship.* The fact that some one—without having sought for it—has not discovered any relationship between certain notes, does not prove that relationship is in abeyance.

Yet Gurney only follows here Helmholtz, who has not found any relationship between such tones.[1] He says:[2] "It is nothing but a step intercalated between two tones, which has no relation to the scale, and only serves to render its discontinuous progression more like the gliding motion of natural speech, or weeping or howling." Now, I have not the least doubt that Helmholtz would really have been delighted to dispense with music 'like the gliding motion of natural speech, or weeping or howling.'

[1] 'Accidentals' these tones are called by the theorists. But in a real work of art there is nothing accidental.

[2] Sensations of Tone, 2d ed., p. 352 b.

Such is the music of cats at midnight, not of mankind. So I need not add anything to this argument.

Stumpf in his most recent publication on Consonance and Dissonance[1] shows that he believes in the dogma of the diatonic scale as strongly as anyone else.

My investigations into musical theory led me to the conclusion, that one of the chief errors is the exclusion of the number 7. Herzogenberg,[2] indeed, tried to introduce the 7 into the theory of music. But just where he thought the 7 to be in place, is this number obviously in the wrong place. My investigations into the question as to whether the 7 really plays a part in music, had already led me to final conclusions, when recently I found, looking by chance at the English translation of Helmholtz's 'Tonempfindungen,' that similar conclusions had been drawn half a century before by H. W. Poole,[3] then an organ builder in Worcester, Mass. In the first two German editions of the 'Tonempfindungen' Poole is not mentioned at all, in the third he is mentioned only as the inventor of a new keyboard, not as the author of a musical theory. Ellis only in his translation of the 'Tonempfindungen' thought Poole's theory at least worth mentioning, although he does not recognize it as true. However imperfect and inconsistent Poole's theory, as stated in his paper, appears, he deserves the credit of having first found one of the most influential obstacles to the progress of musical theory. He seems to me to have made a mistake in so far as he attempted to introduce at once his immature theory into musical practice and spent his time in inventing new keyboards, which are very interesting indeed, but are of little value in improving or developing the theory.

The so-called diatonic scale, which is the basis of all discussions in Helmholtz's 'Tonempfindungen,' was introduced into the modern theory of music by Zarlino in 1558. It was accepted by Rameau in his 'Traité de l'Harmonie,' 1721. According to Rameau (and Helmholtz) no numbers play any part in music but 1, 2, 3, 4, 5 and 6. This is certainly not a law

[1] Beiträge zur Akustik und Musikwissenschaft, 1.

[2] Vierteljahrsschrift für Musikwissenschaft, vol. 10, pp. 133–145.

[3] *Silliman's American Journal of Arts and Sciences*, 1850, vol. 9, pp. 68–83, 199–216.

derived inductively from observed facts, but a dogma, because one may, as Poole rightly states, very easily observe that the 7 acts psychologically in a way corresponding to the action of 2, 3 and 5, whereas, indeed, with other prime numbers, as 11, 13, etc., this is not the case.

Rameau built up the scale in the following way: He started from a certain pitch called tonic, to which in the scale he attributed the number 24. Now, because the tones of the intervals 2 : 3 and 3 : 4 besides the octave 1 : 2 have the closest relationship, he added to 24 the numbers 32 and 36, 24 : 36 = 2 : 3, 24 : 32 = 3 : 4. We shall later see that the mistake here made by him was his starting from mere consideration of harmony, without noticing that in melody the form of succession, the way in which the tones follow each other, brings into existence quite a new factor of the greatest importance. If Rameau had considered this, he would have found that the tonic of a melody can never be represented by the number 24, which contains 3. The same error has entered all theories—as far as I know—up to this time. Even Poole, although he detected the possibility of using the 7, because of his use of harmony as a starting point did not see that it is simply impossible to represent the tonic by a number containing 3, but stated that the old theory in very many cases was right, whereas really there is not one single case where that theory cannot be demonstrated to be wrong.

We saw that Rameau concluded the numbers 24, 32 and 36 to be the most important in his scale. Now, since 24 : 30 : 36 and 32 : 40 : 48 and 36 : 45 : 54 each represent the chord 4 : 5 : 6, he built up his complete scale out of those numbers, only using instead of 54 the half 27 : 24, 27, 30, 32, 36, 40, 45, 48.[1]

From this scale he derived another scale for the so-called music in 'minor' by beginning from 40 and multiplying the first numbers of the 'major' scale by 2 : 40, 45, 48, 54, 60, 64, 72, 80.

[1] Rameau's 'explanation' of the æsthetic effect of the relation of pitches by the mere physical fact, that sounding bodies usually produce, not a single pitch, but several simultaneously (partial tones), does not concern us here any more than Helmholtz's 'explanation' of the same effect by 'identical partial tones': 1, because these explanations do not explain anything but contradict almost more facts than they confirm; 2, because our task in the present paper is only to describe observable facts by laws, but not to deal with hypotheses.

But neither Rameau, nor before him Zarlino, nor after him Helmholtz and his followers have thought it necessary to tell their readers what facts observable in *melody* justified the use of that series of numbers as the basis of a theory of music in general—of melody as well as of harmony. We shall in the following start from melody, because melody is the only essential of music.

Comparing different melodies with the so-called diatonic scale, we notice that some melodies contain fewer, others more notes than that scale. How do these facts agree with the theory that the diatonic scale is the basis of all music? To this question Helmholtz answers: in the first case the composer has not made use of every note, in the second he has added some notes in order to render the music more like howling. The only answer I know is: to base any music on the diatonic scale is a fancy.

For a long time the tempered scale of twelve equal intervals within the octave has been used in music. Rameau, who recommends this scale for practical purposes, claims that any interval of two successive notes in real music, equal to three-twelfths of the octave, *e. g.*, *g*—*a*♯ on the piano, has to be regarded theoretically as the interval 5 : 6, any interval of two successive notes equal to four-twelfths, *e. g.*, *a*—*c*♯ on the piano, as the interval 4 : 5, etc., although there are slight differences. The result of this assumption has been the theory that music is made up out of intervals, *i. e.*, that the intervals between the notes immediately following each other are the essentials of music, causing the æsthetic effect. This theory is supported even by Helmholtz, although it obviously contradicts his theory of the diatonic scale and his doctrine of what he calls 'just intonation.' An example of this theory of intervals is to be found in Helmholtz's statement, that the æsthetic beauty of a melody depends on the number of Thirds and Sixths which it contains, a statement that lacks any foundation, as Gurney has demonstrated.[1] We shall see that music is made up, not of intervals, but of notes and that the relative pitches indeed cause the æsthetic effect, but not only the intervals of every two immediately following pitches.

[1] Power of Sound, p. 148.

There can be no doubt that the tempered scale cannot be made the basis of a theory of music, that theoretic conclusions drawn from considerations regarding the intervals of the tempered scale have no scientific foundation. A scientific theory of music can only be a *theory describing the laws of music performed in just intonation*, but in just intonation that *really* is to be called 'just,' not in that *seemingly* just intonation of Helmholtz, which—as can be proved by experiment—does not deserve this name.

The Æsthetic Laws of Melodies, which contain only Two Different Notes.

When we hear successively two tones, the vibration rates of which are to each other as 2 : 3, or briefly speaking, the tones 2 and 3, we notice something not describable, which I shall call the *relationship* of these tones.[1] To understand what is meant hereby, the reader may listen to the successive tones 7 and 11 or 11 and 10, in which cases he will notice that the two tones have no relation at all to each other. We may describe these same facts in still another way, saying that in the first case (2–3) the two[2] tones *form a melody*, whereas in the other case (7–11) they do not. This expression means the same thing, although different words are used.

Beside the relationship, the hearer will observe in the case of 2 and 3 something else, namely, that after hearing 2 and then 3, he wishes to go back to 2, *i. e.*, to hear 2 once more. On the other hand, when we hear 3 at first and after that 2, we do not wish to hear 3 once more. If 3 and 2 are repeated several times in order to prolong the melody built up out of these tones, we are satisfied in the case where the melody ends with 2, dissatisfied in the case where the melody ends with 3. Save in a few instances, where a peculiar psychological effect is aimed at, no melody *that contains 2* can end with another tone but 2.

[1] In the following I shall try to describe the facts in a language as plain as possible, avoiding all dubious terms like 'fusion' ('Verschmelzung'), all flowery, but meaningless phrases, so common in musical æsthetics, and all the barbarous terms of the ancient and mediæval and even modern theorists, which are a mere burden to memory, without having any scientific value.

[2] For the sake of simplicity we consider at first melodies of two different notes only.

In the case of the tones 3 and 4 the melody must end with 4, not with 3; in the case of 4 and 5 with 4, 5 and 8 with 8, 7 and 8 with 8, 4 and 9 with 4, 15 and 16 with 16; *i. e.*, the *general law* is: "*one of the tones being a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody.*"[1]

When we hear a melody built up of the successive tones 3 and 5 or 5 and 7, we observe a *relationship* similar to that of the cases above, but we notice that it does *not* make any difference whether we hear, *e. g.*, 3 first and then 5 or 5 first and then 3. In neither case do we wish to hear the first tone once more or not once more; or better: such a wish, if any, is not caused by the particular relationship of these tones. This psychological effect is indeed restricted to the powers of 2 (including $1 = 2^0$).

That no relationship at all is to be observed with tones represented by the prime numbers 11, 13, 17, 19, etc., leads to the conclusion that only tones represented by the prime numbers 1, 2, 3, 5, 7 and their composites possess that psychological property. That the 7 cannot be excluded in the case of *two* different notes, does not, of course, decide the question whether in the case of more complicated melodies—as in real music—the 7 really *is* excluded or not. This question can be answered by observation only. So much is certain, that there is no reason for excluding the 7 from the theory of music *a priori*. We shall see later that there are very few melodies indeed which do not contain the 7.

The relationship, which we observe, is closer in some cases than in others; *e. g.*, very close with the tones represented by the numbers 1 and 2, or 3 and 8, less close with the tones 3 and

[1] Lipps (*Psychologische Studien*, Heidelberg, 1885), who describes these facts in a similar, although not in the same, way, has tried to explain why these facts are facts by the hypothesis, that any sensation of tone is not really a continuous sensation, but a series of short sensations interrupted by short empty times. These single short sensations, each corresponding to one single vibration, are to be observed, according to Lipps, only in the case of very low tones. I have shown in my paper, 'Ueber die Rauhigkeit tiefer Töne' (*Zeitschrift für Psychologie*, 13, p. 75), that there is not the slightest foundation for such a hypothesis. Doubtless, regarding explanations of the facts above stated, the progress of the physiology of the nervous system in the future will bring us in a moment farther than any speculation is able to do.

5, or 5 and 7, or 15 and 16. I have tried to bring the different relationships into a series. It seems to me, that the order of relationship within one octave is the following: 1–2, 2–3, 4–5, 5–6, 4–7, 6–7, 8–9, 15–16, 5–7, 5–9. Whether there is a slight degree of relationship in the cases of 7–9 and 14–15, or no relationship at all, I do not venture to decide. Transpositions by octaves do not, according to my observations, cause any difference, so that the relationship of 1–3 is of the same rank as that of 2–3, the relationship of 3–5 of the same rank as that of 5–6. I do not assert that the series above represents quite an accurate order of the relationships. However, whether the series is a little more or less accurate, is of very little importance for the subsequent investigations. I will emphasize further, that when I speak of the *relationship* of two successive tones, the order of succession of the two tones in the melody is not regarded at all.

Similarly, we observe in those cases where one of the two tones is represented by a pure power of 2, that our wish to have this tone at the end of the melody is much stronger in some cases than in others; *e. g.*, very strong with tones represented by the numbers 3 and 2, or 3 and 4, or 5 and 4, less strong with tones represented by the numbers 7 and 8, or 9 and 8. However we may omit here an accurate determination of the place that would appertain to each ratio in a complete series, because this does not appear to be of considerable importance for our present investigations.

One question of this kind we may still consider, namely, whether in the case of the octave, where the tones are represented by the numbers 1 and 2, the psychological effect of the melody's being closed by 1 is different from that when the melody is closed by 2. Lipps[1] asserts that of the two tones 1

[1] *Psychologische Studien*, p. 132: "In jedem Ton ist der Rhythmus seiner tieferen Octave, nicht der der höheren, vollständig enthalten. Ist ein Ton gegeben, so bedarf es zum Vollzug der Empfindung eines nachfolgenden um eine Octave höheren Tones noch der selbständigen Zweitheilung des durch den gegebenen Ton vorgezeichneten Rhythmus. Diese Zweitheilung ist, wie oben gesagt, die einfachste rhythmische Leistung. Immerhin ist sie eine Leistung. Dagegen wird unserem Empfindungs-vermögen gar nichts rhythmisch Neues zugemuthet, wenn die tiefere Octave folgt. Darnach muss der Octavenschritt von oben nach unten in geringerem Grade als der Fortschritt zu etwas Neuem erscheinen und in höherem Grade den Eindruck des sich in sich Beruhigenden,

and 2, we prefer to have 1 at the end of the melody. I could not convince myself of the truth of Lipps's assertion and am inclined to believe that he was deceived when he made this observation, either by having in mind at the same time a certain complicated melody which contains two tonics, but closes on the tonic below, or by—what is less probable in this case, but happens very easily in psychological observations—yielding to his own theory, based on the hypothesis above mentioned, which indeed leads to such a conclusion. I have to confess myself that, if I exclude all psychological effects of tones other than 1 and 2, I cannot detect any difference, whether 1 is at the end or 2. This justifies my regarding 1 simply as a power of 2.

The Complete Musical Scale.

The complete musical scale is the series of all tones, which may occur in one melody, however complex this may be. As soon as we know the æsthetic laws of melodies, which contain only two different notes, it is very easy to construe this scale. Suppose a melody begins with the tone 5, followed by 3. Now we know that if it results in a *melody* at all, the tone 3 can be followed only by a tone that is *related* either to 3 or perhaps to 5 or, if not related to either 3 or 5, perhaps to a further tone that is itself related to either 3 or 5. It is impossible, therefore, that the third tone in this melody should be, *e. g.*, 19 or 33, because neither 19 nor 33 fulfills any of these three conditions. So the third tone can only be a composite of powers of 2, 3, 5 and 7; *e. g.*, 45, which is related to 5 as well as to 3; or 75, which is related to 5; or 135, if this is followed, *e. g.*, by 9, which is related to 135 as well as to 3 and 5. Generally speaking, since relationship of two successive tones is observed only in the cases where the tones are represented by composites of 2, 3, 5 and 7, we have to draw the conclusion, that a complete musical scale cannot contain any numbers except powers of 2, 3, 5 and 7 and their composites. Still, whereas in the case of only *two* different notes the numbers representing possible tones are

also endgültig Abschliessenden machen, als der von unten nach oben. Dass es in Wirklichkeit so ist, kann nicht bezweifelt werden." I regret that from my own observations I am compelled to doubt this.

—according to observation—*restricted* to the numbers of certain ratios enumerated in the above series of relationships, there can of course be no restriction if the melody can contain an unlimited number of different notes. *I. e., the complete musical scale is represented by the infinite series of all composites of the powers of 2, 3, 5 and 7.* We find the beginning of this series in the table on page 251, if we omit the numbers of the lowest line, save 2^0, and read the table from the bottom to the top.

Actually, of course, no music will ever make use of an *infinite* number of different notes; if for no other reason than that the life of a man would be too short for such a performance. We need not, therefore, continue the series farther than actually existing music requires. I have found the series to suffice when continued up to 1024. Besides, in the table, I have omitted of 5 all higher powers than 5^3 and of 7 all higher powers than 7 itself, because I have found no case where these omitted powers of 5 and 7 are used. This is interesting when compared with the fact that of 3 much higher powers are used, and that $9 = 3^2$ appears even in ratios that represent direct relationship, and that of 2 *all* the powers are of absolutely equal value with respect to relationship.

Complete Musical Scale (New Theory).

c# 542.4		d 574.7			d# 608.9	e 645.1			f 683.4		f# 724.1			g 767.1			g# 812.8		a 861.1		a# 912.3		b 966.5			c 1024.0		
7	—	7	7	—	—	7	—	—	7	—	7	—	—	—	7	—	—	—	7	—	7	—	7	—	—	—	7	—
25	5	5	—	—	25	5	5	—	—	25	25	5	—	125	—	—	25	5	5	—	—	25	5	5	—	125	—	—
3	27	—	81	9	3	9	—	81	3	27	—	9	729	3	27	3	—	81	3	27	—	9	27	3	243	—	9	—
525	540	560	567	576	600	630	640	648	672	675	700	720	729	750	756	768	800	810	840	864	896	900	945	960	972	1000	1008	1024
	270	280		288	300	315	320	324	336		350	360		375	378	384	400	405	420	432	448	450		480	486	500	504	512
	135	140		144	150		160	162	168		175	180			189	192	200		210	216	224	225		240	243	250	252	256
		70		72	75		80	81	84			90				96	100		105	108	112			120		125	126	128
		35		36			40		42			45				48	50			54	56			60			63	64
				18			20		21							24	25			27	28			30				32
				9			10									12					14			15				16
							5									6					7							8
																3												4
																												2
525	135	35	567	9	75	315	5	81	21	675	175	45	729	375	189	3	25	405	105	27	7	225	945	15	243	125	63	2°

The numbers in the table, which of course represent *relative* pitch, are brought into connection with an *absolute* pitch by arbitrarily calling c the pure powers of 2. In the line below the musical names are the numbers which—compared with the octave 512 to 1024 in the complete scale—represent the pitches corresponding to the system of 12 notes in equal intervals within the octave, a system that is generally used with instruments of unalterable pitches, as on the piano. Below this line we see the powers of 3, 5 and 7, of which the numbers of each column are composed.

In the following I shall make use of a simplified method of numeration. *All pure powers of 2, as 1, 2, 4, 8, 16, 32, etc., will be represented equally by 2. All composite numbers containing a power of 2 will be represented without the power of 2. So I always shall write simply 5 instead of any of the numbers 5, 10, 20, 40, etc., 9 instead of 9, 18, 36, 72, etc., 27 instead of, e. g., 432, 21 instead of, e. g., 336, etc.* The advantage of this method will show itself.[1] *The bottom line of the table* contains the numbers which are used to *represent* in my theory any of those numbers which are different only by powers of 2, *i. e.*, any of the numbers of the same column.

Analysis of Complex Melodies.

We are now sufficiently prepared to pass on to the analysis of complex melodies. By 'complex melodies' I mean melodies which contain tones not related to each other, or, better, tones related to each other not directly, but by mediation of a third tone, so that, as we shall see, the melody must *theoretically be dissolved into partial melodies*. We shall analyze a number of well-known melodies and derive from them *inductively the laws of construction of complex melodies.*

Our first task must be, to determine exactly the relative pitches which the composer *meant* in a certain melody. The

[1] I ask the reader, if he wishes to understand my theory, to bear in mind that according to this numeration 9 : 3 is identical with 3 : 2, although arithmetically 9 : 3 = 3 : 1. But in my theory it is necessary to regard 1 as a power of 2 and to represent 1—as all powers of 2—by the number 2. Just so is 75 : 15 (which in absolute numbers may represent, *e. g.*, 150 : 240 or 75 : 30) identical with 5 : 2.

common musical notation, the historical development of which must be understood, does not give the pitches exactly, but approximately. In order to determine the pitches meant by the composer, we must have an instrument that gives *all possible* pitches, so that we only have to choose among them. Those organs which have hitherto been built by Helmholtz and others in order to compare Helmholtz's 'just intonation' with 'tempered' intonation, cannot serve our purpose, because they do not contain all possible pitches, as represented in our table of the complete scale. By the aid of Clark University I have been able to construct a reed organ that gives exactly the pitches of the highest four octaves (64–1024) in the table. The absolute pitch of this organ has been arbitrarily chosen and is identical with the numbers in the table representing the relative pitches, so that the lowest reed gives 64 (complete) vibrations in a second.

By the help of this organ it is, in most cases, very easy to determine what pitch is meant by the composer. In very rare cases only is there doubt which of two (or more) possible pitches produces the better æsthetic effect. The reeds, however, have to be voiced carefully, lest the observer should form a wrong judgment through peculiarities of the quality of certain reeds. Besides one must be aware of the tendency to deviate a little from the numerically right intonation,[1] the cause of which has not yet been made quite clear. Such a tendency, however, is not likely to have affected my observations, because the smallest differences of pitch in the complete scale are considerably greater than the average error caused by that tendency.

All melodies represented by musical notes I shall transpose (if necessary) in certain ways, without regard to the notation (the 'key') used originally by the composer. This transposition causes only a difference in the absolute pitch of the melody, a difference which does not at all concern us here. The reader will see at once that this transposition enables us to use in any example the same *musical name* for the same *number* representing the relative pitch.

[1] C. Stumpf und M. Meyer, Maassbestimmungen über die Reinheit consonanter Intervalle. Zeitschrift für Psychologie XVIII., pp. 321–404; and Stumpf's Beiträge zur Akustik und Musikwissenschaft, Heft 2.

Before I begin the theoretical analysis of complex melodies, I shall give in musical notation with the theoretically corresponding numbers an example of a simple melody, *i. e.*, a melody, any tone of which is related to any other tone. Our example is taken from Beethoven's 'Fidelio.'

I. BEETHOVEN, FIDELIO.

Above the notes are set the theoretically corresponding numbers. According to my method of numeration two different pitches are represented by the number 2. These *pitches represented by the number 2 I shall call 'tonics.'* So our melody starts with a tonic, and passes to 3,[1] a movement which causes a strong wish to go back to a tonic. This wish is not granted, but the melody passes to a closely related tone 5, that strengthens the wish to go to a tonic 2. The melody then, indeed, goes over to 2, but only in order to leave it again and to pass by 5,

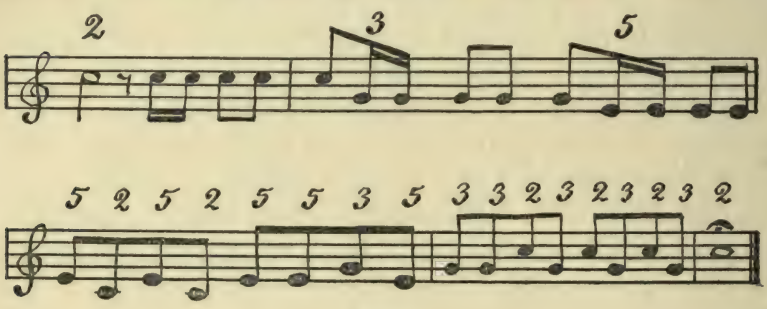

FIG. 1.

that causes rather a strong wish to go to a tonic, to 3, that causes a very strong wish to go to a tonic. However, this wish is not satisfied. The melody passes again to 5, back to 3 and now to 2. But 2 is left again for a short time and replaced by 3, whence the melody finally surrenders to 2. This last movement over 3 back to 2 is repeated twice. 2 has won the battle and the hearer is æsthetically satisfied.

Such a simple melody as that above is not often used in music. It serves, because of its simplicity, very well as a signal, for which, indeed, it is used in Beethoven's opera. A complex

[1] To this pitch 3 I do not give any name, least of all the name 'dominant,' because such a name cannot be scientifically justified.

melody is the following tune of Silcher to Uhland's poem 'Der gute Kamerad.'

II. Silcher, Der gute Kamerad.

The melody starts with 3. The hearer of course—granted that the melody is unknown to him—cannot guess whether the pitch heard is a tonic or not. From 3 the melody passes to 2. Now the hearer wishes to remain at this pitch.[1] But the melody passes to 5 and from there to 9, which is related to 5 and 2, although not very closely. From 5 as well as from 9, more from the former, less from the latter, the hearer wishes to go to 2. This wish is granted, but after some time has passed 2 is left again and is replaced by 3. For a short moment the melody passes back to 2 and thence to 9; from 9 not back to 2, but to the related tone 5 and from 5 to the related tone 3. Although 3 is not 2, some rest on 3 causes the hearer a certain amount of satisfaction, because the 9, which he has met several times, is to 3 as 3 : 2 (according to my numeration). A number like the 3 in this case I call a *secondary tonic*. From 3 the melody passes to 21, which is to 3 as 7 : 2 (*i. e.*, 3 appears as a secondary tonic), so that the hearer wishes to go back to 3. Instead of such a movement the melody passes to 5, which is closely related to 3, but not at all to 21. We shall see that this is very common in music, viz., that *the melody instead of passing to a pitch, to which the hearer expects it to pass, passes to another pitch, but one closely related to the expected pitch.* Now, with 5 the melody cannot end, because the tonic 2 has been heard already, and in such a case the hearer expects the melody to end with a tonic; otherwise he would not be æsthetically satisfied. After a series of further movements the melody indeed ends with 2.

We may simplify this melody by reducing it to the relative pitches of that melody of Beethoven, 2, 3 and 5. The first part of Silcher's melody is then represented by the numbers 3, 2, 5, 2, 3, 2, 5, 3, 5, forming by themselves an unfinished melody. From the remaining pitches with 3 we get the following

[1] I hope that no one will raise the objection, that the hearer will probably suppose the melody to be yet unfinished and so will certainly wish to hear the rest.

melody: 3, 9, 3, 9, 3, 21, 3. Why I have added to the remaining pitches the last 3 will be seen at once. If we divide each number by 3, we get 2, 3, 2, 3, 2, 7, 2, a complete melody with tonics, ending with a tonic (which in the compound melody I call a '*secondary tonic*'). This second melody is interwoven with the first one, 3 in both cases being of identical pitch. We have to lay stress on the word 'interwoven,' because in books on musical æsthetics we often find an opinion formed, as if a melody were always composed of smaller parts by simply filing

FIG. 2.

them together. The possible ways of combining melodies such as those above, are of course almost infinite in number, and we cannot discuss all possibilities here. One point only will I mention. I added to the second partial melody the last 3 in order that this melody should end with its tonic. In the compound melody it is not necessary that this last pitch should be really heard; it may be replaced by a closely related pitch (5), which must be a pitch of another partial melody (in our example:

of that partial melody that carries the tonic of the whole). Similar instances are innumerable in music. We shall see that there is a general law, that *the last tone of a partial melody may be left out, if it can be replaced by a tone closely related to the tone omitted.* This however is but a special case of the more general law mentioned on page 255.

Below the notes in our example II. the numbers are to be found that represent the same melody according to the old theory (Zarlino-Rameau-Helmholtz). Yet here also the powers of 2 are omitted, in order to make the numbers more easily comparable. We notice that in the old theory a pitch is represented by 2, which is no tonic, but may even be left out entirely without changing greatly the character of the whole. But the tonic itself is represented by 3, and the melody ends with 3. According to that theory we have not several partial melodies, one interwoven in a highly artistic way with another; but we have simply a number of pitches, arbitrarily taken from a ' scale' and composed—not into a melody, of course—but into a succession of pitches, in the same manner as a bricklayer builds a wall out of bricks, where it is quite indifferent whether he takes first the one and then the other; or the reverse. When I say ' not into a melody,' I do so, because *the most elementary psychological law of melodious succession by that theory is simply neglected,* viz., the law, that no hearer is satisfied, if after having heard once or more the tonic 2 he does not find 2 finally at the end of the melody.

When we compare the numbers of the new theory with those of the old one, we see that in the new theory the value of most of the numbers is one-third of that of the numbers of the old theory. In order to compare the intonation corresponding to the numbers of one theory, with the intonation corresponding to the numbers of the other, we may multiply all numbers of the new theory by 3. Then—in our example II.—the numbers of both theories are identical, except in the case of *f*, which in the new theory is represented by 63, in the old theory by 64. Now, it is easy to make the experiment of playing the same melody with 63 (according to the new theory) and again with 64 (according to the old theory). In the first case the

melody sounds all right. In the second case the hearer has an impression similar to that experienced when he looks at a painting totally misdrawn. As soon as one hears 2 (64) he expects this to be a tonic, but his wish to have the melody end with this tone or an octave of it, will never be satisfied. The succession of pitches does not end on the note that appeared by its intonation to be a tonic, but on another one. And this is called 'just intonation' by Helmholtz and his followers.

However, Lipps has gone still farther and pretends to have derived from that 'just intonation' a general psychological law, viz., the law, that from the preceding 2, the tone 3 becomes 'in a higher degree the proper aim of the movement.'[1] This is similar to saying of Napoleon: from his previously being emperor, 'Elba' became in a higher degree his proper aim. In order to support his theory Lipps cannot bring forward any argument but the following: "Wenn zwei sich streiten, freut sich der Dritte," 3 in this case being 'the third.' However right this proverb in general may be, it has no place here, because a fight of sensations of tone has never been observed.

One of the errors of the common theory is the presumption, that each note of a melody (save those which only serve to render the music more like 'howling') must have a direct relation to the tonic. Yet the 21 in our example has no direct relation to the tonic 2. Of course, 21 could not have been used in this melody but for its relation to 9 and its still closer relation to 3, which is to 21 as 2 is to 7. That the note, represented in the new theory by 21, has no relation to the tonic, has, indeed, always been recognized by unprejudiced musicians. Since the acceptance of Zarlino's diatonic scale they wondered only how this was possible, although the interval was a 'fourth,' represented by the ratio 3 : 4. They did not see that what is called a 'fourth' in music, *is not one interval, but several quite different ones, sometimes 3 : 4, sometimes 16 : 21, sometimes still others.* We shall see in the following, that music makes extensive use of pitches which have no relation to the tonic, but which, together with one or more pitches related to the tonic, form a partial melody, interwoven with the other partial melody that contains the tonic.

[1] Th. Lipps, *Psychologische Studien*, p. 135.

III. Beethoven, Das Blümchen Wunderhold.

The above tune of Beethoven's begins with 3, 5, 2. Then follows 27, which has no relation to the preceding tonic 2, although it does have to 3, the first note of the whole melody. This 27 following 2 affects the hearer like an enigma, because of the lack of relationship. The solution of it is given at once by 9 and 3, which form a melody with 27 (3 being a secondary

Fig. 3.

tonic) and which at the same time are closely related to the preceding melody 3, 5, 2. A detailed description of the following is scarcely necessary. The reader will find there musical forms similar to those in the previous examples; similar forms, but not exactly the same, for even such a small number of different pitches allows an almost infinite number of melodious combinations.

IV. Irish Folk-Song.

The above melody may be easily analyzed by the reader, except that part where g♯ occurs. Our table shows for g♯ the numbers 25 and 405. When we play the melody with the one or the other of these tones, we have a decided preference for 405. With the succession of the tones 2, 15, 27, 405, 27, 15, 2 the melody obtains quite a peculiar color. From the first mentioned 2 the melody passes to the related tone 15, so that the hearer wishes to return to 2. The melody does not re-

Fig. 4.

turn, but passes from 15 to the related tone 27 (15 : 27 = 5 : 9), *i. e.*, to a tone that has no relation to the expected tone 2. Moreover, on 27 as a secondary tonic the partial melody 27, 405, 27 (separately 2, 15, 2) is built up, of which tones none has a relation to 2. The psychological effect of all this is similar to that of a dangerous situation out of which one sees no way. Yet very soon there appears relief. From 27 the melody passes back again to the related tone 15, whence it returns to the tonic 2.

The melody is followed by the series of tones in musical notation, of which tones it consists. The cross below g ♯ means here as in all cases, where it occurs in the following, that the corresponding pitch—according to the old theory—has no relation to the melody, but ‘ only serves to render the melody more like howling.’ A further proof of the worthlessness of that theory is to be found in the fact that this note—as we saw above—has a certain definite intonation, whereas a slight difference of intonation could not have any considerable effect, if the note were only to render the melody ‘ more like howling.’

A remarkable fact is that this melody contains no 7.

V. Mozart, Don Giovanni.

In this case the melody begins with a tonic, but the first part (four bars) ends with 3. This would not be remarkable at all in itself, but the way by which the melody arrives at 3 shows

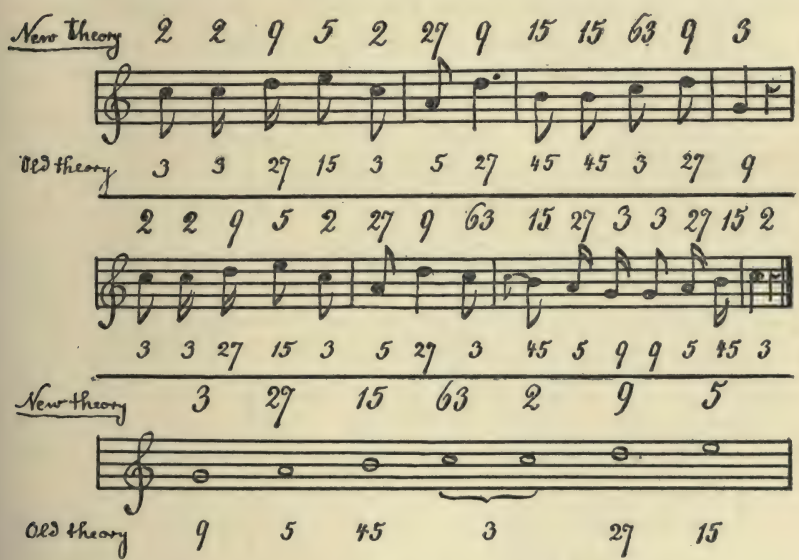

Fig. 5.

that it originated in the mind of a master. From 2 the melody passes to 27, that has no relation to 2. This movement affects the hearer in the manner mentioned with example III. After 27 we hear 9, which leads us nearer to the tonic. Now we

might replace all the notes in the third bar by 9 and so pass directly from 9 to 3, still nearer to the tonic. Here Mozart introduces the partial melody 15, 63, 9, 3 (not 15, 2, 9, 3, as the theory of the diatonic scale supposes), where 3 is a secondary tonic, so that this partial melody separately may be represented by 5, 21, 3, 2. That the whole of the first four bars ends with the secondary tonic 3 of a partial melody gives the hearer simultaneously some satisfaction and dissatisfaction: satisfaction, because of the partial melody ending with its tonic; dissatisfaction, because this part of the whole ends with a strongly accentuated tone, that is not the tonic of the whole, but on the contrary the tone 3, which causes a particularly strong wish to go to the tonic 2. Now, only, is the final return to the tonic capable of producing the immense æsthetic effect of this melody of Mozart's. The old theory does not tell us anything of all this. According to that theory, the whole beauty is caused simply by the pitches being taken from the diatonic scale. Then indeed it is difficult to understand why other melodies, the tones of which are also taken from that scale, are not just as beautiful.

VI. Beethoven, Sonata, Op. 14, No. 2.

The first part 3, 3, 75, 5, 15, 2 of this melody ends with a tonic. It contains the partial melody 75, 5 (15, 2) with 5 as a secondary tonic. The second part is a partial melody (separately represented by the numbers 3, 3, 9, 75, 15, 2), built up on 9 as a secondary tonic. This partial melody itself has to be analyzed again, the melody 675, 135 (separately 5, 2) being a partial melody with 135 as a secondary tonic. The old theory represents the second part of Beethoven's melody by 5, 5, 15, 2, †, 27.

In the third part the melody, passing through a number of smaller partial melodies, touches 2 several times and finally closes with 2. The last notes are 3, 21, 15, 2. There we may question why the 3 above the 2 has been used and not the 3 below the 2. Some theorists have alleged that the mere movement of ascent and descent of the pitches, even without any relationship, is an important factor of the æsthetic effect of music. It

is possible, indeed, that ascent and descent have a slight æsthetic effect. However, I have not observed this effect myself nor has any one else, and so I must regard its alleged importance as lacking any proof. What I have observed, is the following: Suppose the composer intends to form a melody of two different tones, one of which is absolutely given, *e. g.*, 2 in a certain pitch, but the other is determined only by its number, *e. g.*, 3,

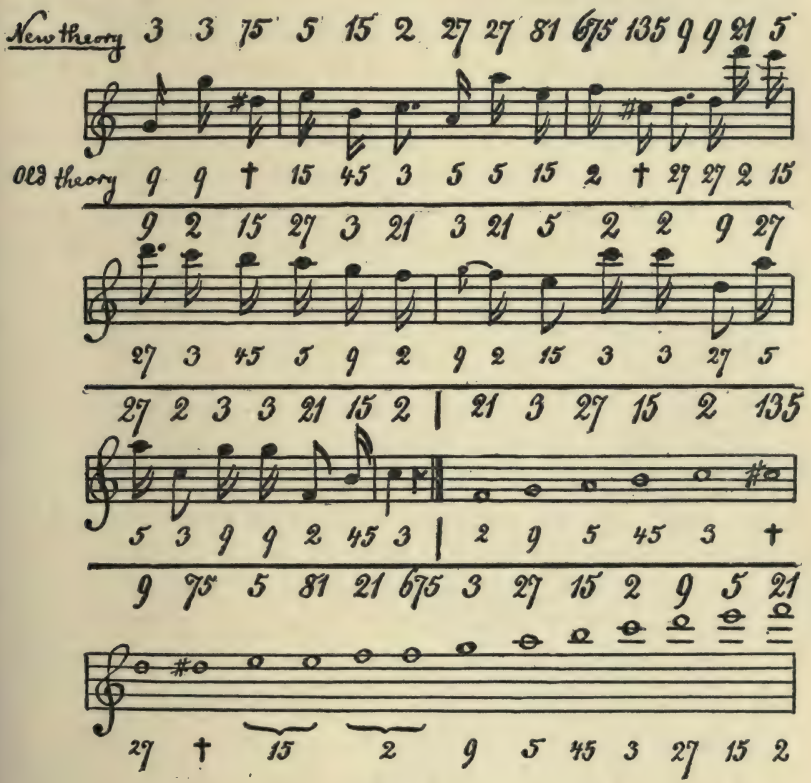

FIG. 6.

so that several pitches, differing by the interval of an octave, are possible. In such a case, if there is no particular purpose, that 3 is used which is *nearest the given* 2, *i. e.*, the 3 below. Yet when not only the 2 is given, but another tone too, *e. g.*, the 5 just above the 2, and 3 (without determined pitch) has to be added, that 3 is added which is *nearest both given tones; i. e.*, the 3 above the 2 is added. When in this latter case the 3 below

2 is used, the melodious unit of the three tones 2, 5 and 3 is solved and two partial melodies (composed of 2 and 3 and of 2 and 5), interwoven with each other, are the result. When the composer desires a complex form, he has to use in this case the 3 below, when he desires a simple form, the 3 above.

If Beethoven at the end of the present melody had used the 3 below the 1, the whole of the last four notes would act upon us as a combination of the two partial melodies 3, 15, 2, and 3, 21, 3, the latter interwoven into the former. But in the present form the last four notes act upon us as a combination of the partial melodies 3, 2, and 21, 15, 2, because 3 by the greater distance has been severed from 15 and 21. The second partial melody 21, 15, 2 in this case is again composed of two partial melodies, viz., 21, 15 (separately 7, 5), and 15, 2, so that, by this use of the 3 above, the whole form of these four notes has been changed.

Another case of this kind is the separation of the 21 from the *preceding* 9 in the third bar. The 21 forms a partial melody with the *following* 9 of the fourth bar.

Gurney says in his 'Power of Sound': "the ascent from the dominant to the tonic above, the descent to the tonic below, each seeming right in its place, while in a form that was worth anything either would be resented as a substitute for the other." We have seen above that this fact is not so wonderful and beyond human understanding as Gurney assumes it to be. Of course, it is not impossible to exchange a 3 or *any tone* of another number *for an octave of it;* yet in some cases the melody ends better with a simpler, in others better with a more complex form.

VII. Beethoven, 4. Symphony.

In the above melody I wish to call the reader's attention to the melodious form 5, 45, 45, 3, 25 of the third and fourth bars, which form, according to the old theory, would be nothing more than 'howling.' This form has to be regarded as composed of the melodies 5, 45, 3 and 45, 25 (separately 9, 5). The following 27 is then connected with this form by the relation of 27 to 3. I have tried on the organ above mentioned to

find whether in the case of g♯ 25 is right, or 405, which with the following 27 would form a melody (separately the melody

FIG. 7.

15, 2). 25 appears to me to yield a better æsthetic effect than 405.

VIII. BEETHOVEN, 6. SYMPHONY.

The first part of this melody is a partial melody (separately represented by the numbers 2, 3, 5, 2, 3, 5, 2, 3, 5, 2, 3, 2) with 27 as a secondary tonic. The second part is a partial melody (separately 3, 2, 2, 3, 2, 2, 3, 2) with 9 as a secondary tonic, so that the movement from the first secondary tonic to the second is identical with a passage from a 3 to a 2. The next partial melody 9, 15, 9, 9 (separately 3, 5, 3, 3) is a melody without a tonic. From this we arrive at the last part, which contains the primary tonic 2. The first three bars of this part are composed very simply of 2, 3 and 5. Three times the

melody touches 2; but when 2 is expected again for the fourth time, 21 is heard, which has no relation to 2. The psychological effect of such a movement has already been mentioned. Yet 21 is followed by 9, which is related to 21 as well as to 2. This melody of Beethoven's is an excellent example for those

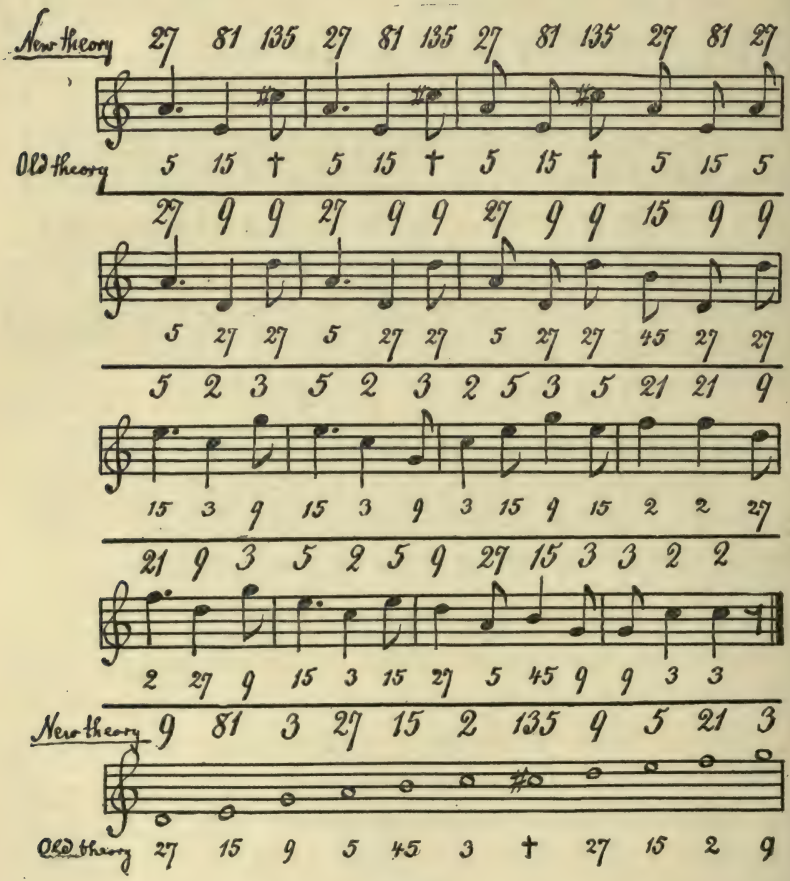

FIG. 8.

readers who wish to convince themselves of the truth. I can only advise them to play this melody in the intonation corresponding to the new theory and to compare it played in Helmholtz's 'just intonation,' which—when the hearer is aware of the intonation—annihilates the whole æsthetic effect.

IX. Schubert, Heidenröslein.

Schubert's 'Heidenröslein' is a very complex piece of music. The first part is a comparatively simple melody, which ends with a tonic. The next part contains 9 as a secondary tonic and is separately identical with 9, 21, 5, 9, 2. The third part contains 3 as a secondary tonic and is separately identical with

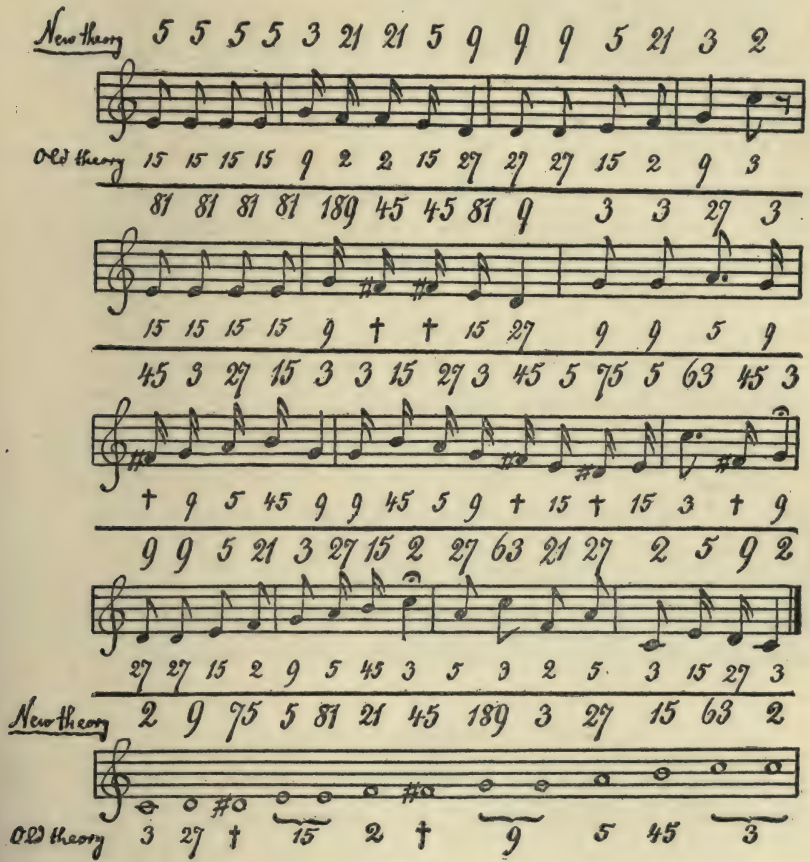

Fig. 9.

2, 9, 2, 15, 2, 9, 5, 2. This 3, the secondary tonic of the third part, is to the primary tonic 2 in closer relation than in the previous secondary tonic 9, so that after having left the primary tonic 2 for 9, the secondary tonic of the second part, we gradually approach the 2 again. The fourth part is a melody with-

out a tonic, but begins and ends with 3, so that it is closely related to the third and fifth parts. The fifth part leads back to a tonic 2. The sixth part begins with some tones not related to 2, which remind us of the second, third and fourth parts of the whole melody. These tones are connected with the final tonic by means of the following 9. The old theory, of course, cannot tell us whence originates the extraordinary æsthetic effect of this comparatively brief song. The old theory is unable to detect the complexity of form of this melody, which distinguishes it from trivial melodies, the simple forms of which, heard a thousand times, we get tired of.

Yet for a song that is really so complex as this one of Schubert's, the accompaniment is of great importance. I have found indeed that people, who were not sufficiently trained to pay attention to the accompaniment, did not appreciate the æsthetic value of Schubert's song. However, this paper being devoted to melody only, I will not go into discussion of the harmonies. I hope I shall be able to publish my investigations into harmony soon in another paper.

We shall analyze now *a second, rather different class of melodies, viz., melodies without a tonic.* Instances of such melodies have been found already in some partial melodies within the preceding examples. In this second class of music we find many instances of partial melodies containing a secondary tonic, but we do not find any primary tonic 2.

X. German Choral.

In this melody there is no pure power of 2, *i. e.*, no primary tonic according to our definition of a tonic. But there are secondary tonics. The melody has been harmonically treated indeed, by musicians, as if 63 were a tonic (identified with 64). However the æsthetic effect of the melody itself is destroyed by this, although I will not deny that many a hearer in spite of the destruction of the melody may be much pleased by the successions of harmonies offered by the composer instead of the melody.

15 5 9 63 15 27 15 45 3 3 45 5 45 5
3 45 9 5 45 3 9 9 5 9 63 63 15
3 45 27 3 45 5 45 15 63 15 27 9 15
27 15 63 9 5 45 3 27

FIG. 10.

15 5 45 3 15 15 5 15 5 45 3 15 15 5 5
3 3 63 9 15 15 27 3 27 15 45 25
5 45 3 25 27 15 63 9

FIG. 11.

XI. Wagner, Lohengrin.

The above well known melody from Wagner's Lohengrin is another example of a melody without a (primary) tonic. This melody ends with 25. The previous melody ends with 15. We shall find other melodies end with still other numbers. There is not such a law, that a melody without a tonic must inevitably end with a certain number, as in the case of a melody with one (or more than one) tonic, where the last note of the melody must be a tonic.

XII. Old German Song.

This melody ends with 5. The old theory, which calls the last note of any melody the 'tonic' or 'key note'—although there is no complete agreement on this matter—would regard 5 to be the key note in this case; and since the Major Third (25) of 5 does not appear in the melody, but the Minor Third (3)

Fig. 12.

does, the old theory would say that this melody is a melody 'in minor.' Now, according to the theory mentioned above, which Herzogenberg has brought forward, the interval of the Minor Third of the key note would have to be represented by 3 : 7. The whole melody then would be represented by the fol-

lowing numbers: 27, 9, 5, 21, 9, 135, 9, 5, 9, 27, 21, 5, 21, 9, 9, 135, 9, 5, 9, 135, 9, 5, 9, 27, 21, 5, 21, 9. I have tried in this case as well as in others the intonation corresponding to Herzogenberg's theory. The result of this examination is, that I have no doubt that Herzogenberg's theory in general is wrong.

XIII. Lithuanian Folk Song.

I tried first to represent the above melody by numbers in such a manner that the last note corresponds to 2. Yet the melody sounds out of tune in that intonation. The intonation that corresponds to the above numbers appears to me to be the right one. So this melody proves to be a melody without a tonic, ending with 9.

How the lack of a primary tonic in the melodies of the second class, the want of a tonal basis at the end of the melody,

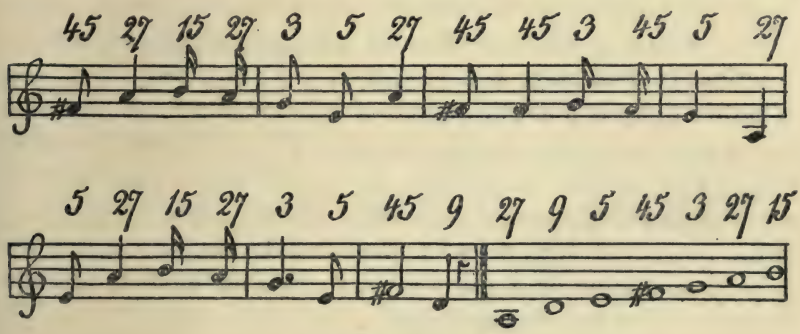

Fig. 13.

acts upon the hearer, is well known by every music lover. This peculiarity of the æsthetic effect of these melodies is readily understood from our theory. It cannot be understood from the old theory.

I regret that I cannot add to these examples an Arabian melody containing a 'quarter tone.' Many Arabian 'scales' with a quarter tone are to be found in different books. Yet these scales are worthless for the science of music. I have been unable to detect any real melody containing a quarter tone, although there is no doubt that such melodies are used in the Orient. According to my theory there is nothing wonderful in the fact of a quarter tone in a melody. *E. g.*, a melody built up of the

tones 35, 9, 5, 21 may contain a 'quarter tone' of the interval 35 : 36.

Summary.

1. When we hear a succession of different pitches we are affected in a certain way, which cannot be described, but has to be regarded as an elementary psychological fact. We may express this fact by saying: we observe with different ratios of the vibration rates a different relationship of two successive pitches; in certain cases we observe no relationship at all.

2. We call the relationship close when the ratio of the vibration rates consists of small numbers; remote, when the ratio consists of large numbers. In this respect, however, we should bear in mind that any power of 2 has an exceptional position.

3. The order of relationship within the series of related pitches seems to be the following (using my numeration above described): 2–2, 2–3, 2–5, 3–5, 2–7, 3–7, 2–9, 2–15, 5–7, 5–9. Whether there is a slight degree of relationship in the cases of 7–9 and 7–15, or no relationship at all, I do not venture to decide. Neither do I assert that the series above represents an accurate order of the relationships. In all other combinations of the powers of 2, 3, 5 and 7 there is only an indirect relationship by mediation of a third tone, so that we have to dissolve the melody into partial melodies.

4. No relationship—as far as I know—has ever been observed by any human being in the case of prime numbers higher than 7, as 11, 13, 17, 19, etc. Relationship is to be observed only with pitches represented by the relative numbers 2, 3, 5, 7 and their composites.

5. When of two different pitches forming a melody one corresponds to a pure power of 2 and the other to one of the numbers 3, 5, 7, 9, 15 (neglecting all powers of 2), we wish to hear 2 at the end of the melody, and we are æsthetically dissatisfied until our wish is granted. The power of 2 in such a case we may call 'tonic.'

6. All melodies, simple or complex, are to be divided into two classes, those with a tonic and those without a tonic.

7. Complex melodies, as commonly used in music, may be composed of simple melodies either by simply filing them together or by more or less artistically interweaving them into each other.

8. Very common in music is the particular æsthetic effect produced by the following movement: The melody instead of passing to a pitch to which the hearer expects it to pass, passes to another pitch, but one closely related to the expected pitch. A special case of this movement is: the last tone of a partial melody being left out and replaced by a related tone.

9. Of all tones ('octaves') that have a certain relation, aimed at by the composer, to a given tone, that tone is used which is nearest the given tone. The effect of another 'octave' being used is: the change of a simple melody into a complex one.

10. The 'quarter tones' of the Arabs are no exception to the general psychological laws of melody.

11. No observation has been made that would justify the exclusion from the theory of music of the number 7. On the contrary, observation proves that the exclusion of the 7 is impossible.

12. The musical terms 'Major,' 'Minor,' 'Dominant,' 'Sub-Dominant,' have only a historic, no scientific value. Neither have the ancient and modern 'scales' any scientific value.

INDIVIDUAL TESTS OF SCHOOL CHILDREN.

BY E. A. KIRKPATRICK.

In connection with tests made upon about five hundred school children to determine defects of sight and hearing, I made a few other tests that may be of interest to this Association, which has devoted considerable time and some money to formulating a series of individual tests for college and university students, with the idea that a test of ability, better than an examination test, or supplementary to such a test, may be found. Such a test is needed more for public school children than for College students, and it is reasonable to suppose that accuracy and rapidity in the simple sensory and motor activities would be a better indication of general ability in school children than in College students, whose activities are more complex and less concerned with simple sensory and motor operations. The results of the preliminary tests thus far made, which are to be followed by others, are not conclusive, but only suggestive of what is required in the way of standards and of what may be hoped for in the future from such tests.

The tests used were: First, counting aloud as rapidly as possible for ten seconds. Second, making vertical marks as rapidly as possible for the same length of time. (The time it took the pupil to count the marks he had made was also recorded.) Third, sorting twenty-five cards into four piles according to oral directions by the numerals one, two, three and four. Fourth, sorting twenty-five cards into four piles according to visual impression, each card having on it one of the four letters A, B, C, or D. Fifth, naming four ink spots, about one minute being allowed in which to name them. Time was measured in all cases by means of a stop watch and each pupil was tested but once after a few preliminary tests, in which it was found that in most cases the time was not greatly different in the second test unless the experiment was misunderstood. All

the tests were made by myself, hence the personal element which would be quite important in the oral card test and in giving directions was the same for all. A very few pupils who had just entered the first grade could not count far enough to consume the whole of the ten seconds. With very young children the mental element of association apparently determined the limit of rate, while for older pupils the rate of movement of the vocal organs seemed to fix the limit of speed. In the marking test the younger children were usually more careful as to how they made the marks than the older ones. In the card test also it was at first surprising to find that fewer mistakes were often made by the younger than by the older children, but observation showed that the younger children were likely to think more about the thing to be done and the older ones about doing it quickly, hence one took the sensory form of reaction while the other inclined toward the motor form and therefore sometimes made more mistakes.

More than half the children tested were American, though many other nationalities were represented. They all belonged to the model and practice schools of the Fitchburg Normal School, and at least one model school and one practice school are represented in each grade tabulated except in the second.

There were considerable differences both for individuals of the same room and for rooms of the same grade, but, in general, as is shown in the table, there was an improvement in the first four tests up to about the sixth or seventh grade, when it ceased or became slower or there was an actual decrease in rate.

In the fifth or ink spot test the younger children seemed more suggestible or imaginative, as they named more of the spots. The number who saw the spots as objects was less in the fourth, fifth and sixth grades, but increased in the seventh and eighth grades, though not equalling those of the first. The younger children seemed to have no doubt whatever of the spot being a picture of the object they named, while the older children simply said 'it is some like' or 'it looks a little like' 'a dog,' 'cloud' or whatever else was suggested. This supremacy of the small children is striking when we consider that

the number of mental images that they have is much smaller than that possessed by older children, who may name a part of the body or the map of a country or something else that the younger children know nothing about.

TABLE.

GRADE.	1	2	3	4	5	6	7	8
Number of children,	36.	16.	51.	72.	67.	35.	49.	55.
Average number counted,	25.8	31.	31.	34.5	36.1	45.	40.	37.8
Average number of marks made,	19.7	28.	35.	36.8	38.8	45.8	46.	47.4
Time of sorting cards (oral),	52.	43.	49.	35.	29.	31.	24.5	28.
Time of sorting cards (visual),	61.4	40.	41.	34.	25.3	27.3	23.6	23.
Pictures named,	2.9	2.5	2.6	1.8	1.9	1.7	2.1	2.2

The smaller number of objects seen in the spots by the children of the fourth, fifth and sixth grades is probably to be explained by the fact that children of those ages have become more critical in their sense perceptions as their ideas have become more definite and as they have learned from life's experiences and from training to be more careful in their judgments. The older pupils of the seventh and eighth grades, on the other hand, have passed into another stage in which they realize that a picture is not necessarily this or that, but may resemble any one of several things, hence they are not afraid to say what it looks like.

Most of those who named three or four of the spots named more than one object of a class, as two kinds of vegetables or two animals, showing that the presence of one idea of a group made others of that group more suggestible.

The results of the various tests were not tabulated according to age, but the averages of those who were much older and those much younger than the average of the room in each case were taken and found to differ somewhat from the general average of the room. In general, the pupils older than their grade were better in their tests in the lower grades and not so good in the higher grades, while younger pupils were better than their mates in the eighth grade and not so good in the lower grades. The two factors apparently determining the record a child makes are experience that comes with age, and ability. In the lower grades

the experience that comes with age is more important than intellectual brightness and knowledge, while above the sixth or seventh grade, at which time the powers tested seem to reach their maximum, ability counts for more than age or both count in the same direction to increase the comparative record of bright young children who have been promoted with older pupils.

In order to determine the significance of these tests of individual pupils, the teachers were asked to grade the children into three classes according to ability as compared with their mates in the same room. In most cases the teachers could do this without much hesitation, though pupils who were good in some lines and poor in others and pupils who did not always try gave them some trouble. The children were graded by me according to the tests and the two gradings compared. A different standard was adopted for each room for each test except for the ink spots and the perfectness of execution in making lines and sorting cards without mistake, in which cases the standards were the same for all grades. For the average grade of ability a standard was adopted with such a maximum and such a minimum above and below the mean or average for children of the same room and sex, that half, or a little more, of the children would be in the average class, while approximately one-fourth were in each of the two classes above and below the average. The teachers' gradings were compared with the grading according to the counting and marking test combined, compared with those of the two card sorting tests combined and finally compared with all the tests combined. The percentage of correspondence did not differ greatly in the different comparisons, but was a little greater for all the tests than for part of them. Fifty-seven per cent. were graded just the same by the combination of all the tests as by the teacher, while only two per cent. were placed in the class farthest removed from that in which they were placed by the teacher. This is probably as close a correspondence as would obtain between the judgments of two different teachers, which indicates that such tests would be likely to be of some value to superintendents in settling doubtful cases of promotion.

Where the results of the tests are directly opposed to the

teacher's judgment of a pupil we cannot say which is most nearly correct, but the following principles should be considered. In general, average size or ability in vegetables, animals and men is regarded with more favor when it is a question of selecting for breeding purposes or for positions of responsibility. Unusual development in one direction is to some extent abnormal and often associated with some weakness in other directions. For this reason a good average in such a test is probably as good an indication of good general ability as a record very much above the average. On the other hand one who is much below the average is not only abnormal, but abnormal in the wrong way, and hence cannot be expected to have good general ability. A poor pupil may, therefore, make either a very high or a very low or even an average record in such a test, but a very good pupil is not likely to make a very low record, though he may make only an average one.

One result of this experiment suggests possibilities, and, to my mind, probabilities of the greatest importance in establishing standards for testing individuals of different ages and in determining the best kind of tests to be used for any age. I refer to the fact that in most of the experiments pupils of the eighth grade made little or no better record than those of the sixth and seventh and sometimes not so good a record. Wolfe noted a similar fact in his experiments in that the pupils of the eighth grade judged size and weight more accurately than university students. Bentley also found more improvement in rate of naming words or letters than in naming colors or pictures, while Gilbert in his numerous experiments upon school children found that the rate of improvement in different lines was not the same at different ages We know also that, according to the general laws of habit, improvement becomes slower with practice as the limit of possible improvement is reached. The experiences of every-day life and the school training received by all probably mature some powers before there is occasion to exercise others a great deal. Again, all child study investigations indicate that there are inner laws of development which cause certain activities to become prominent and develop almost to maturity before others have scarcely begun, with almost as much certainty as

the flower of a plant becomes perfect before the fruit begins to form. This inner tendency of development which recapitulates in part at least the history of the race is the principal influence in determining the direction of activity, while the opportunity for practice and laws of habit determine the extent to which the powers are developed. This view being accepted, it seems altogether probable that we are wrong in assuming that increased age should show increased power in elementary activities, and that superiority of one adult to others in these activities is an indication of greater mental ability. In children the possession of great sensory and motor power may be the best indication of general mental ability, but it may be that such superiority in adults should in some instances be interpreted as a sign of arrested development, for the adult who is still developing his sensory and motor activity is perhaps quite inferior to the one who has ceased to develop in those lines, but is now gaining in power of abstraction and reasoning. Good answers to questions calling for facts of general and incidental observation, such as "How many wings has a fly?" "Which way do the seeds of an apple point?" "How many windows are there in a well-known room?" and others like them asked by Professor Cattell may indicate, not so much alertness and mental ability as diffused attention and lack of the power that goes with concentrated and selective attention. We cannot tell *a priori* what good records in the various tests that we apply mean. It may mean that the individual is superior to his fellows of the same age in activities naturally prominent at that age, and hence probably superior in general ability, or it may mean that he has not yet begun to develop along new and higher lines that are appropriate to his age and that are now absorbing the attention of his fellows.

I wish to emphasize to this Association the importance of testing persons of different ages and seeking for the normal standard at each age before we can intelligently interpret individual records. The psychological problem of tests of general mental ability cannot be solved till the psychogenetic problem of the stages of improvement in the various lines of mental development has been solved. In my judgment the results of the tests that have been made for several years upon the entering

students of several universities will be valuable in determining normal standards for the ages and sexes tested, but they cannot be interpreted as measures of general intellectual development and ability till the same tests have been made upon children of different ages.

In conclusion, I would suggest that it is desirable to have tests of such a nature that they can be taken by children as well as adults, that they shall be such that all persons tested will have had about equal opportunity for the exercise of the power tested, and that in the interest of economy of time the tests so far as possible shall be so planned that they can be given to a whole class or school at once, instead of to each individual separately.

DISCUSSION AND REPORTS.

A PNEUMATIC SHUTTER FOR OPTICAL EXPOSURES.

In the course of work concerning the more complicated mental processes the need is often felt for a simple optical shutter, which will be at the same time economical and thoroughly workable. Of the various forms of such shutters hitherto devised the pneumatic is undoubtedly the best; but the majority of its varieties are so complicated and expensive as to make it highly desirable to obtain a simpler form of apparatus, especially when the progress of simultaneous investigations makes the duplication of the shutter necessary.

The mechanism of which a cut is given below has been devised for use in the Harvard laboratory in connection with an investigation in which it was necessary to expose at the same time an object of moderate size and its name in printed letters. The apparatus is inexpensive, and can be made in any laboratory workshop in the course of two or three hours.

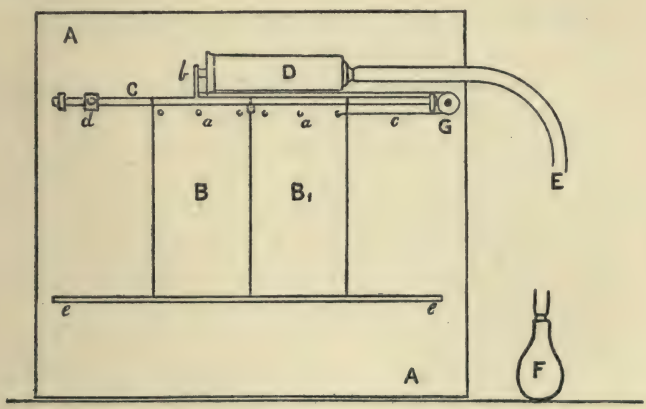

FIG. 1.

The body of the shutter consists of an upright rectangular sheet of thin wood (*AA*), sixteen inches long by fourteen wide, and faced with black cardboard. In the centre of this screen is a square aperture six inches in diameter, and having its sides parallel with those of the

screen itself. To the back of this sheet of wood is attached the mechanism of the shutter. This consists, in the first place, of two rectangular screens of black cardboard (*BB*), each three and one-half inches wide by six and one-half high, which are carried by sections of brass tubing, of one-eighth inch inside diameter, to a projecting flange of which they are rigidly attached by screws (*aa*). These pieces of tubing are mounted on a horizontal brass rod (*C*), one-eighth inch in diameter, on which they move freely. Above this carrying rod and parallel with it is clamped an ordinary medical syringe (*D*), having a plunger with a plain pencil-end. To the nozzle of the syringe is attached a flexible rubber tube (*E*), connected with a large-sized pressure-bulb (*F*).

This syringe furnishes the motive power of the shutter, applied in the following way. To the carrier of the one shutter-leaf (*B*) is soldered a strip of stiff brass (*b*), which projects in front of the plunger of the syringe. As the plunger is driven forward by the pressure of air in the syringe, therefore, it carries this half of the shutter with it. At the same time the action of a silk thread (*c*), attached to the brass strip (*b*) at one end, and to the second half of the shutter (B_1) at the other, and passing over the pulley (*G*), draws the leaf (B_1) in the opposite direction. The aperture thus produced is six inches in height and in width is twice the distance travelled by the plunger of the syringe. The automatic closing of the shutter upon the release of the pressure-bulb is obtained by connecting the two leaves of the shutter by an elastic rubber band passing around the screw heads (*aa*). The movement is centralized by a block (appearing in the cut at the top of the line marking the meeting of the two halves of the shutter) which at the proper point arrests the further motion of the shutter-leaves. The rapidity of the closing movement may be controlled by changing the strength of the elastic band. The width of the aperture is controlled by means of a sliding top (*d*) on the brass carrier-rod, which is fixed in position by a set-screw. The bottoms of the shutter-leaves are carried in guides (*ee*).

The mechanism has been found to work very satisfactorily. The movement is easy and rapid, the time—and of course the extent—of the exposure are nicely controllable, and the apparatus is applicable to all work except such as may require a centrifugal action of the leaves. It is especially convenient for work on perception, visual memory, association and all cases where numbers, words, sentences and the like are to be exhibited. For such use the screen is rotated so that the edge which in the cut forms the right hand side of the ap-

paratus becomes its base, and the movement of the shutter-leaves is vertical instead of horizontal. The aperture then becomes a horizontal slit six inches in length, and of a width varying according to the needs of the experiment, and controlled by the position of the stop (*d*). For second series exposures, when an upper row only of letters or figures is to be exposed after a double row has been exhibited, the silk thread may be slipped off the pulley, when the leaf (*B*) alone will be moved by the action of the plunger, the return being brought about as in the previous case by the elastic connecting band.

A still simpler form of exposure, which may be found adequate for some kinds of work, involves practically only the cost of the syringe itself. A tight-fitting cap of brass tubing is made for the end of the plunger. The projecting half of the tubing is sawed down at opposite sides and pinched flat, forming a spring clamp, within the jaws of which cards bearing numbers, letters and the like may be slipped and held firmly. To use the mechanism it is necessary only to clamp the syringe vertically behind a screen and in such a position in relation to it that the top of the card to be exposed comes, when ready for exhibition, in a line with the bottom of the aperture in the screen. Exposure is made as before by pressure upon the bulb, the card itself rising into view and disappearing instead of being exposed by a moving shutter as in the previous case. The return of the shutter downward is brought about by the air-pressure in the syringe, or, if the piston work stiffly, by an elastic rubber band passing through the end of the plunger and bound to the barrel of the syringe.

In closing I wish to point out the applicability of the syringe to experiments on the dermal senses. For this purpose the syringe barrel is clamped rigidly in a vertical position, the instrument with which contact is to be made is attached by thumb screw or other device to the end of the plunger, and application is made by simple compression of the air-bulb. Such a method has several distinct advantages over exploration with an instrument held in the hand. One of these is the possibility of giving the successive stimulations with local exactness, a condition scarcely to be looked for by the hand method even when the positions are permanently indicated by marks on the skin. The tremor of the hand is got rid of, and the application is clean, firm and even. It will probably be found also that the time of stimulation is more controllable.

The only instance of the employment of an analogous method of which I am aware is that reported by Krohn (*Journ. Nerv. and Ment. Diseases*, March, 1893) in experiments on simultaneous touch

stimulations, in which he used Marey's tambours, to the faces of which were attached the instruments of contact.

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REMARKS ON TIME PERCEPTION.

The opening article in *Mind* for January, 1900, reviewing the contradictory results of Schumann and Meinong in experimenting on time perception in relation to memory-image, is of interest, but fails to note the fundamental difficulties of the subject. In the first place, we must remark on the confusion of objective and subjective time, as when Schumann (p. 3) takes a 'note heard for one second,' a 'tone sensation of one second's duration,' as a standard of subjective value. But to work on a purely subjective problem from objective artificial time is plainly confusing. If we are to understand the content of time perception we must study introspectively and genetically. What are the origin and development of time perception under natural selection?

An organism, pressed by hunger, senses food, and the interval of steps toward the realization is felt as timeful. If realization is absolutely immediate, there is no time sense, as seems often to be the case with infants; but even with them delayed realization of satisfaction brings time perception. From the moment that appearance is seen to be only the sign of reality, that is from the origin of knowledge, we thus find time perception accompanying the progressive steps of realization. Time perception is then bound up with delay and the use of means, and is implied in skill, patience and perseverance, the cardinal intellectual virtues by which organisms save themselves and make progress in the struggle of existence. The genesis then of the time perception under natural selection is in a felt relation of the individual to its environment, and it is a complex of sense of unrealization and of position with reference to realization. And it is also plain, *e. g.*, with infants, that time perception is not experience of mere succession, but of delayed succession which comes with sense of unreality. And time perception is seen to be an adaptive psychosis to exigencies, giving foresight and preparedness, and reducing fright to caution.

The rise and growth of time perception are then involved in expectant attention which implies memory and the cognition of the break between the real and the ideal—that real and ideal are not coincident factors. We may consider that with very young animals and children time perception is solely confined to a purely subjective feeling when food is brought to them. How time perception is related to expectant

attention is easily observed in common experiences. In the train the other day I was not aware of any particular time perception when stopping at a station, but when I watched the objects fly by as the train increased speed, I noticed that time perception became very acute with the increased rapidity of expectant attention, and *vice versa* as the train slowed down.

In this case and others it is plain that the amount of time perception in a given amount of objective time—say one minute—is determined by the rate of consciousness itself. A very simple experiment shows this. While in your usual frame of mind, with consciousness at its usual pace, thinking over a common matter—*e. g.*, planning some ordinary experience, as a trip to the country—estimate the watch time; then, while in the same mood, estimate while taking exercise, as running up and down stairs. With one person, in whose introspective honesty I had confidence, and who did not count and was entirely unsuspicious of the object in view, the acceleration was remarkable. It required 150 seconds actual time for the 90-second time estimate when at rest, while during and after exercise the 90-second estimate was given after only 55 seconds actual time. He ascribed the variation, when he felt 55 seconds to be as long as 150 previous, to increased rapidity of consciousness. And under primitive conditions, when no objective time is known, as with most animals which probably do not appreciate even the natural day as objective time-length, this subjective time increases with expectant attention and rapidity of consciousness. So when a man is drowning his consciousness is forced to top speed, like a lightning-shutter, and time perception is most acute. (The effect on consciousness itself of reiteration in consciousness under expectant attention is time-perception as feeling of experience-room.) When on the train the speed reaches the limit of my perceptive power as to distinct objects, the time perception is keenest, and if it goes beyond, and the landscape becomes mere blur, time perception is lessened and even lost.

Pure subjective time is then a mode of introspection and self-consciousness, and probably the earliest form. Experience here is reflex, first turns upon itself, is aware of itself in deferred realization. The dragging interval is measured by how many consciousnesses active and potential go into it, as unrealizations. Thus to the very thirsty invalid the interval while the nurse is bringing drink is felt in terms of the number of unrealizations in the eager pulse of the waiting consciousness. So the stolid invalid would have little time perception. Notwithstanding that the rate of consciousness determines amount of time

perception, yet it is plain to simple introspection that any given act of consciousness is *per se* timeless, is merely the time unit or point, just as spaceless points compose space. Then there is no 'duration-block' as 'unit' of our perception of time (James), nor can we admit Schumann's simple feeling of duration. Just now I hear a bell tolling, and I listen to the note as a continued vibration, the whole being felt as a specious present; but a little analysis shows that the time-fullness is felt only as experience-room for quite a number of simple immediate consciousnesses, that is, of passing units. If I perceive a musical note as a whole of time—a duration—it is as a continuance through a number of consciousness beats which are each subjectively timeless. The phrase 'as quick as thought' could have no meaning except through objective time. Every duration is such only as a whole of felt successions actual or potential. This does not deny, of course, that the ear-grasp of time wholes is real, and with natural and trained musicians doubtless the feeling is apparently of simple duration, the process of potential units being subconscious. And so when James says ('Psychology,' I., p. 630): "And since we saw a while ago that our *maximum* distinct intuition of duration hardly covers more than a dozen seconds (while our *maximum* vague intuition is probably not more than that of a minute or so), we must suppose that this amount of duration is pictured fairly steadily in each passing instant of consciousness by virtue of some fairly constant feature in the brain-process to which the consciousness is tied," we see here the confusion of subjective and objective time, as if each psychic pulse were apperceptive of itself as taking seconds of time, whereas time consciousness has a very considerable development in purely subjective form before it ever apprehends objective time in any form, and especially in seconds or fractions. And I cannot find in my introspection that the psychic moment ever directly apperceives itself, but I see each psychic beat as a timeless instantaneous act, yet the sum of the acts constituting subjective time. Hence the feeling of immense duration with drowning people when an immense actual and potential experience-room is felt, in fact a life-room.

As to the specious present or 'now' we must, as implied from our previous discussion, dissent from James' *dictum:* "But the original paragon and prototype of all conceived times is the specious present, the short duration of which we are immediately and incessantly sensible." The shortly to be, and the shortly has been, interpreting the former, are much more primitive. The sense of 'now' has no direct simple value for struggle of existence, and is reflective. Any unity of experience is interpreted as a 'now,' as in hearing a long-drawn

whistle or musical note of even quality; and if the whole of life experience were an absolute unit and uniform, and so no break of reality and ideality, we could have an eternal 'now.' The specious present, of course, appears as a sum of possible psychic beats; it is not the elementary timeless component of time, but is given as duration, as a block of experience.

It is important, as we have said, to keep in mind the distinction of subjective and objective time, to note that the psychics of time and the psychophysics are distinct areas of investigation. The rise and development of objective time as well as that of subjective time can only be determined by very delicate and thorough observation and experimentation on infants and animals, reinforced by study of adults, extremely little having been accomplished along these lines as yet. And what stage in mind and why under natural selection do changes in environment, such as day and night, suggest definite intervals of time? That many domestic animals have the feelings of time by some objective notation has seemed probable to some, yet it may be that they fix upon some sign and association merely, and do not mark real intervals. The horse which quits work at the noon whistle does not regard the whistle as a time limit, but merely as a sign of food and rest. Of course in the primitive form objective time may arise merely as keeping tab on the subjective time of expectant attention, and the regulation of subjective time to normal objective time relations is plainly of high utility. Thus the infant suffers much from strained attention, because you cannot make it understand that five minutes are required to heat its food. So animals may come objectively to regulate subjective time. Again, you look at your watch five minutes before the curtain rises at the theatre, and you have in the succeeding waiting a constant flow of both subjective and objective time. The beat of consciousness tends to assimilate to objective beats, as the ticking of a clock may seem either faster or slower than the present consciousness, too slow, say when I am waiting for a train, too fast when in quiet reflection; yet the tendency will be to synchronize subjective and objective time. You feel at the theatre a great length of time, and you keep correcting this by seconds and minutes subjectively estimated, so that you know with practical certainty when the five minutes are up, though you have in that time had more time perception than perhaps in the whole of the previous week. From the psychological point of view, of course, the subjective time is the real time.

The basis of time-perception we have seen to be psychological reflex in the discordance of the actual and real. So physiological time

as from pulse, respiration, etc., is a mode of objective time, and quite late in development; for young animals and children who are keenly alive to time in expectant attention are far from being physiologically observant. Normal life in early stages gains little from physiological reflex, but a consumptive would be likely to note respiration time. Nor is physiological time the basis of our estimation of artificial time in hours and minutes, as we see plainly in the Magee case (*Amer. Journ. Psychology*, VII., 450). He, while out with a camping party, keeps mentally the objective time almost to the minute, and can report whether waked from sleep or climbing a hill the hour and minute correctly, in all of which pulse, respiration and leg time are highly variant. It is obvious in this case, at least, that, as timer of athletic events, his consciousness has formed the habit of objective time-keeping in second beats, minute beats, etc., in a purely direct way without physiological basis, and thus the counting time goes on subconsciously. And here then is an example of subconscious time estimation to which we call the attention of the Editor of *Mind*, who dissents as to evidence.

And it is plain that if all time-pieces were suddenly destroyed, such men as Mr. Magee would become human time-pieces, and would find their function and existence in this time-knowledge, and for scientific purposes, *e. g.*, astronomical and physical, there would tend to arise time-keeping species of men; and as the demand ever became keener for accurate and delicate time-keeping there would be response in the evolution of human society. That is the limit of the *minimum* consciousness beat as time-measurer can not be defined *a priori* physiologically, but is controlled by demand in the competition of existence.

What then is the significance of indifference point? Does not this suggest to physiology, as to pulse? There may very well be a coordination of artificial and physiological time, but this does not prove primitive state or genesis. But the objective time which is most constantly estimated correctly might be, say with Mr. Magee, quite different from the 75 seconds in difference point. Mr. W. S. Johnson's 'Researches in Practice and Habit' (p. 52) conclude against indifference point. There must be a much wider range of experiment with all kinds of men and animals in all kinds of physical and mental states to get real light on this question of indifference point.

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PSYCHOLOGICAL LITERATURE.

La Doctrine de Spinoza exposée et commentée à la lumière des faits scientifiques. Par ÉMILE FERRIÈRE. Paris, Felix Alcan, Éditeur. Pp. ix + 357.

The list of works already published by M. Ferrière reveals a catholicity of tastes and interests rather depressing to the man who feels that he can do good work only by circumscribing the sphere of his activities and becoming more or less of a specialist. Darwinism, The Medicinal Plants of Burgundy, The Apostles, Paganism among the Hebrews, Scientific Errors in the Bible, Biblical Myths, The Soul and the Functions of the Brain, Matter and Energy, Life and Mind,—these are among the subjects upon which he has found it possible to write '*un fort volume.*' From a work on the etymology of four hundred Christian names used in France, he passes to a study of Spinozism. He admits frankly that it will appear strange in the eyes of the general public that, at the close of a century devoted exclusively to money and material pleasures, a man should be found so devoid of sense as to devote himself to philosophy, above all to Spinozism; but, he exclaims, with a noble indifference to consequences, "cela me laisse tout à fait indifférent." The theories of Spinoza are to be condensed into a few propositions, stripped of all obscurity, and so placed before the reader that a 'coup d'oeil' will suffice to reveal both the details and the system in which they find their unity. At the same time a running commentary will turn upon the Spinozistic doctrine the searching light of contemporary science.

But even a cursory examination of this volume will suffice to console those who feel sensitive regarding their inability to write treatises on Darwinism, the Apostles, the virtues of Medicinal Plants, and the Scientific Errors of the Bible. M. Ferrière need feel no apprehension lest those, who are in a position to judge, accuse him of having devoted an inordinate amount of time to the study of philosophy. His material he has gotten largely from Saisset, and he has not improved it in working it over. From Spinoza himself it is clear that he has gotten little, and he has largely misunderstood that little. Of the

philosophies which preceded Spinozism, and without an understanding of which it is impossible to follow intelligently the reasonings contained in the 'Ethics'—of these, M. Ferrière is palpably ignorant.

The puzzling question of the relation of substance to its attributes, a question over which students of Spinozism have expended no little thought, M. Ferrière settles summarily with an illustration, a 'fait scientifique.' Plaster is composed of sulphuric acid and lime. Here we have a case of substance and attributes. Suppose an infinite universe of plaster; the lime may be regarded as representing the attribute extension, and the sulphuric acid as representing the attribute thought. The combination of the two gives us the one substance with its attributes. *Voilà tout!* (pp. 29–30). Attributes are the *elements* of which substance is composed (p. 32). The difficulty of making plaster of any sort out of such diverse elements as extension and thought were conceived to be in the seventeenth century does not present itself as a difficulty to M. Ferrière's mind. Again, modes are distinguished from attributes in that they are necessarily finite (p. 44). Did M. Ferrière ever read Letter 64 in Spinoza's correspondence? Has he pondered over the significance of the 'fixed and eternal things' of the treatise 'De Intellectus Emendatione'? Still again, it is insisted that Spinoza was an out and out nominalist (pp. 63–73), and as the Spinozistic doctrine of essences is quite overlooked, it is but natural that M. Ferrière should be wholly at sea when he comes to discuss the immortality of the mind. It seems to him that, in this doctrine, Spinoza has arbitrarily stepped beyond the limits of his system, and simply taken up with the beliefs of the vulgar (p. 128). To one who reads Spinoza in this way, the 'Ethics' is and must remain a sealed book, a mere mass of incoherencies.

It is not necessary to criticize M. Ferrière's volume in detail. There is a scientific side to the Spinozistic philosophy which is well worthy of attention even in our day; but it cannot be properly treated by one who has never penetrated Spinoza's thought at all, and whose book fairly bristles with statements which can be categorically denied by citations from Spinoza's writings. If the volume on the Medicinal Plants of Burgundy contains as many errors as this one, it must have been the unhappy occasion of many a colic and of much Gallic profanity. As for the Apostles, if they have been misrepresented as has Spinoza, they have just grounds for an action at law against the author.

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La Nouvelle Monadologie. Ch. Renouvier et L. Prat. Paris, Colin et C^ie^. 1899. Pp. 546.

Of the dual authorship of this work it would be difficult to find any evidence in the work itself beyond the title-page: the hand may be the hand of Prat, but the voice is the voice of Renouvier. At the close of a long life, the accomplished and venerated master of an influential philosophical school sums up, in collaboration with one of his disciples, the positive elements of his teaching in a compact system. Such appears to be the historical significance of this important treatise: it contains the constructive doctrine of the French critical philosophy.

This doctrine allies itself by its very title with the 'dogmatic' metaphysics of Leibnitz. In spite of the many elements derived from Kant, the method is also to a large extent what Kant would have regarded as dogmatic. The groundwork of the system, closely examined, is the affirmation of the theses, to the exclusion of the antitheses, of the Kantian antinomies. Thus it is held (1) that the world is composed of simple substances; (2) that it is finite; (3) that it contains free agents; and (4) that it originated in the creative act of a supreme First Cause. And the logical principle on which each of these propositions rests is that which essentially underlies Kant's argument for each of the theses of his antinomies, namely, the impossibility of thinking the actual number of terms in any given series as infinite.

It is impossible, however, to accept these results on this ground. The abstract principle is indeed unassailable: an actual number cannot be infinite. But this principle, when applied to the series of conditions thought of as determining any conditioned element in experience, conflicts, as Kant showed in the arguments for the antitheses, with another, the necessity, namely, of always going beyond any given term n of the series to the next, $n + 1$. Now our authors, following Aristotle, explain the appearance of an antinomy here by distinguishing between the actual and the possible number; thus, we are told, the number of actual events which have occurred in the world is finite, but there is no assignable limit to the number which may hereafter take place. But why not? If time can have a beginning, it may also have an end. The possibility, therefore, of an endless succession of events in the future is not real and concrete, but merely the abstract possibility of thinking the progressive series continually added to. But this is precisely the case with the regressive synthesis of past events as conditions for any given event. For it is impossible to conceive of any event, however remote, as without a precedent event as its condi-

tion. Our authors deny this: the cause of an event, they say, need not be an antecedent event. But even granting that there are causes of events which are not events at all, neither antecedent nor coexisting, that which determines any event to its place in the time-series must be an antecedent event; for time, in its aspect of succession, is merely the abstraction of the succession of events. If we think of the time before any given event as empty, if, *e. g.*, we try to think of a first event, we hypostasize an abstraction; and the same difficulty occurs if we try to think of the beginning of time, for the first beginning of time would be itself an event. Our authors object that to think the series of past events as infinite is to suppose them illusory, which is repugnant to good sense. But surely it is the nature of the reality of the time-series which, among other things, this dilemma of thought brings in question.

Two cardinal ideas in the system are individuality and freedom. The existence of simple substances is regarded as a datum implied in the existence of compound substances. It is rightly inferred that a simple substance cannot be extended; hence it is determined as a monad by internal or qualitative relations. But what is the ground of the argument from the existence of compound substances as given to the existence of simple substances as also given? Not, surely, the correlativity of the terms, for on that ground we might also conclude from the existence of the relative the existence of the absolute, an inference which the authors reject. Hence the logical ground can only be, it would seem, here as in the previous case, the impossibility of thinking an actual series unlimited—the argument for the thesis of the second antinomy. But what compound substances are taken as given? Surely not substances compounded of monads, for this would beg the whole metaphysical question. Extended substances, then, *i. e.*, bodies. But if bodies are held to be compound substances, it is because they are regarded as composed, not of immaterial entities, but of smaller bodies. And this leads to the endless series—the argument of the antithesis.

The same principle—the contradiction in the conception of an infinite series—is urged in support of the doctrine of freedom, and also in support of what might seem to imperil it, the doctrine of a creative First Cause. But in the matter of freedom, at any rate, the authors advance beyond this argument; for, after defending ably and at length the possibility of freedom, as over against universal mechanical determinism, they finally assert the fact of freedom as a 'rational belief' motived by its appeal to the sentiment of duty and the interests attaching to personality. This puts the belief on substantially the Kantian

foundation. The discussion would have gained by a clearer exposition at the outset of the conception of freedom, which seems to fluctuate between that of an originative, absolutely spontaneous, cause of phenomena, the power of contrary choice and the hegemonic function of the intelligence. But probably all three moments are meant to be included.

The above are among the principal topics discussed in the earlier parts of the book, entitled respectively, The Monad, Organization, Mind, Passion and Will. Towards the end of the last named part the 'rational belief' in freedom and morality is connected in the practical reason with the belief in goodness and in individual and social ideals and is made the ground of belief in the moral perfection of God. This leads to the discussions in the remaining parts under the headings, Societies and Justice. The central theme here is the contradiction between the above-mentioned beliefs and the actual order of things in our experience. The pessimistic aspects of the world are depicted with startling effect. It is maintained that this world is radically incompatible with the realization of our ideals, that a perfect society on earth is not merely morally, but physically, impossible, no matter how long time we allow. But a world thus unjust in its very constitution cannot, it is held, be the work of a perfect Creator. The problem then is to explain this present so evil world in agreement with the principles of the monadology and with the postulates of the practical reason. The theory advanced is in substance as follows: The monads were originally created perfect and formed together a perfect world. There was no superfluous unorganized matter, but only such as was necessary to supply the needs of organisms, and the physical medium in which the latter lived was an elastic fluid of a density appropriate to their use. The physical forces were disposable in the most useful manner at the will of man. The families of men possessed the highest social organization, and, except perhaps in the case of plants, there was neither generation nor decay. This state of things was brought to an end by the selfish exercise of free-will. This destroyed not only the social, but the physical harmony. The evil, small at first, gradually grew, till at length, in a war of titans, the primitive world went to smash. The nebulous matter with which our present world begins was the result of this stupendous break-up. In this matter were contained the germs of the later developed organisms, psychically one with the monads of the earlier time. Subject to the laws of the new order, subject to birth and death and to all manner of physical and social inequalities, the human race is here suffering retribution and

undergoing discipline. The justice in each individual life must be assumed, but cannot be seen because of the complexity of the conditions, past, present and future. The process of retribution and education is to continue, here or elsewhere, till, in the remote future, timed presumably to the completion of the work, the catastrophe foreseen by science brings to a close the present world-order in an immense conflagration. Then "new heavens and a new earth wherein dwelleth righteousness." Such is the authors' conception of the drama of the world as it is unfolding itself through the acts of free agents in accordance with the consciously predetermined creative plan.

Comment on this conception, the likeness and contrast of which both to the Leibnitzian Theodicy and to the views current in orthodox Christian theology are manifest, would here seem to be superfluous. The surprising thing is that all this audacious speculation should be made to appear as reasonable as it does. Some of its most startling features seem to follow as legitimate deductions from the principles developed in the earlier part of the work, while others present themselves as plausible interpretations of many of the facts of experience. But this is very different from a metaphysics that moves in the sure path of science. Speculatively, the radical defect of the scheme seems to be that God and the created monads are brought into no sort of intimate relation. A finite number of monads created a vast number of ages ago are left to work out, without miracle, their own destiny. Why, under the given terms, God should not interpose on occasion, is not evident. If, on the other hand, God is thus otiose so far as the destinies of the world are concerned, it is not apparent why he is needed in the scheme of things at all. Why not regard the monads as individually eternal beings? The truth, however, is that the method by which the monads are themselves posited is vitiated at the start by dogmatic assumptions. It refuses to accept the lesson of the antinomies and it ignores the method of transcendentalism. But this latter method also has not succeeded in convincing the world that it leads to the solution of the ultimate question concerning the relation of the universe to the individual, of the one to the many, and in the end it must be confessed that a book like this, which is honest and able, contributes to our interest both in philosophy and in life, even though we may believe that our best hope of results lies along other paths of investigation. It will at least help to stimulate the new interest in personality.

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Interprétation Sociale et Morale des principes du Développement Mental—Étude de Psycho-Sociologie. J. M. BALDWIN. Traduit sur la seconde edition anglaise par G. L. DUPRAT. Paris, Giard & Brière. 1899. Pp. vi+580.

This is a translation of Professor Baldwin's 'Social and Ethical Interpretations.' It is, indeed, to be regretted that Professor Baldwin's remarkable work should have gone into French at the hands of so careless an interpreter as M. Duprat appears to be. His knowledge of the English language does not seem very thorough. We have noticed, in fact, gross mistakes in the translation. To give one instance, M. Duprat translates 'fruste' for 'crude' (p. 9), which is just the contrary of what the author means ('crude' means immature and 'fruste' worn out). We have noticed words which are not French, *e. g.*, 'réflexive' for 'reflective' (pp. 217, 219, 241 *et passim*). In various places the author's thought is so obscured that one must turn to the original in order to understand what is meant. M. Duprat has also a peculiar way of solving the difficulty of rendering into readable French the somewhat complex style of Professor Baldwin; he suppresses entire passages. We have noticed that in different parts of the book.

Why has the title of the book—already somewhat lengthy in English—been 'stuffed' with superfluous words in the translation, thus rendering it longer and less comprehensible? Is not 'Aspect Éthique et Sociale du Développement Mental' a simpler and clearer way of rendering the original title in French than 'Interprétation Sociale et Morale des principes du Développement Mental'? The reader will easily see that the word 'principes' is superfluous and misleading.

As is usual with European books, especially French, there is no index, and the original contents table has been shortened to indicate only the heads of the chapters, suppressing any mention of the sections as given in the American edition. This means unavoidable and vexatious delays in the perusal of the book.

In conclusion, we do not believe that this poor translation will help to vulgarize Professor Baldwin's doctrines among French students. Doubtless, many of them will find the author's English more intelligible than M. Duprat's French. Decidedly, M. Duprat is just the kind of translator to whom the Italian epigram might be applied: "Traduttore, traditore."

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ANALYTICAL.

Ueber Gegenstände höherer Ordnung und deren Verhältniss zur inneren Wahrnehmung. By A. MEINONG. Zeitschr. f. Physiologie und Psychologie d. Sinnesorgane. Aug., 1899. Pp. 182–272.

The discussion forms another chapter in the long controversy respecting the difference between the perception of a whole and an aggregate of perceptions. It takes the form of a polemic against a previous article of Schumann's in the same Zeitschr. (B. 17).

Schumann and Meinong may be taken to stand respectively for two opposing methods whose results, not only in this but in many other connections, are frequently brought into contrast. Schumann would seem to start with the question: What is the difference *in content* between the perception of a whole and the aggregate perceptions of its parts? Meinong, rather, with the question: What is the difference *in meaning* between a whole and the sum of its parts? And then, feeling that 'meanings' must also be represented among our mental contents, he searches within these for the one that stands for unity. So that for Schumann, the whole problem is an empirical task set for introspection. For Meinong, too, the search among contents is introspective; but the analysis that precedes it imposes a certain condition upon the content sought—that of being inseparably connected with the constituents whose unity it is. Meinong's success, it would seem to the reader, turns upon the clearness with which he can make it appear that *any* content could fulfill such a condition.

As one might anticipate, Schumann finds nothing contained in the perception of a unit save the several perceptions of its constituents *plus* such of our reactions to the stimuli as result only from their combined action. "To form a unit-whole means at bottom to act as a whole (on a percipient)" (Zeitschr. 17, 135 f.). Such 'action as a whole' is exemplified in the feelings and associated images produced by a melody, no fraction of which results from a single note struck alone. It is this content that remains constant when the melody is played in another key, and which leads us to pronounce it the same melody.

The view which Schumann is here opposing, and which Meinong defends, is that of Ehrenfels (Vierteljahrschr. f. Wiss. Phil., 1890). There are present, thinks Ehrenfels, in the perception of a unit, certain contents not resolvable into any that Schumann suggests. These he calls 'Gestaltsqualitäten' and defines them to be "such unitary mental contents (Vorstellungsinhalte) as are connected with the presence in consciousness of perceived complexes which themselves consist of separable elements" (loc. cit.).

As an isolated statement of doctrine the term 'Gestaltsqualität' and its definition just given would fit Schumann's view about as well as Ehrenfels'. Both assume that the perception of a unit involves something more than the aggregate perceptions of its parts. And to this 'something more' is relegated the function of explaining why we detect sameness of 'form' in two complexes whose constituent elements are quite unlike. The only question would seem to be the empirical one—are these 'form-qualities' further analyzable?

But back of these like-sounding formulæ is a profound difference of motive to which Meinong gives expression. In fact, if it were merely a question of finding a 'common element in change' Schumann's feelings and associated images might answer. So might an ether-wave or a cannon shot. But whatever relation, causal or otherwise, these might bear to our judgment of the likeness of two complexes, they would not be *what we mean by* the unity of each of these complexes (232 f.). And here the difference in method shows its effects.

What conditions must this unity-content fulfill if it is to exhaust our meaning when we regard a group as a unit? Just as a relation is built upon its terms and is not to be thought without them, so a unit (Complexion) is founded on its parts, and whatever content corresponds to its unity cannot be thought independently. Both of these (relation and unity) are called 'objects of a higher order' whose characteristic is to be 'built upon other objects as indispensable presuppositions' (190). The two are indeed 'partially coincident' in meaning, for 'relation is the complex from the point of view of its constituents . . . the complex is the relation and its terms taken together' (194).

It is quite evident that the accuracy of Schumann's introspective powers is not the real point at issue, for no such 'gewöhnliche Vorstellungen' as he is looking for could satisfy the conditions of requiring other perceptions as 'indispensable presuppositions' and of belonging to one group of these and to it alone (237). The question is whether any term that does fulfill these conditions can remain (like Ehrenfels' form-qualities) 'Vorstellungsinhalte' or even (like Meinong's objects of a higher order) 'objects' of inner perception.

Of course much depends on the definition of inner perception. Meinong recognizes this and devotes the second chapter of his article to a survey of the field of introspection. He proceeds, by an appeal to introspection, to show that introspection informs us of many more elements of our mental life than Schumann is supposed to admit. It informs us not only of our perceptions of 'physical objects,' but also of

psychical objects (memory images), mental activities (judging, desiring, willing) and finally of such 'ideal' existences as are most relations and units. It is doubtful whether Schumann would recognize himself in Meinong's account of his position. It seems less doubtful that Meinong has stretched his own definition of inner-perception beyond the breaking point when he comes to take an introspective account of stock. If "a thing can be said to be perceived (wahrgen.) only when its existence is immediately known, *i. e.*, 'known without reference to any other knowledge which may in any sense serve as a premise' (212), how can an 'object of higher order' be said to be perceived when its essential nature 'is to be built upon other objects as indispensable presuppositions' (190)? Immediate knowledge must be understood in so broad a sense that it excludes practically nothing in the way of interpretation that may be required to account for the meaning a given content possesses when set in the system of our experience.

From all this discussion the reader emerges with the impression that neither side has made a clear analysis of its own motives. Each argues as though a question of fact were at issue, whereas the question that needs an answer is not: What is *the* fact? but: What is *a* fact? The answer belongs to a theory of experience. Meanwhile it seems fairly clear that Meinong's motive for insisting upon the factual character of 'objects of higher order'—their accessibility to mere observation (even though introspective)—springs from a desire to escape from an appeal to 'nominal-definitions' to account for the relationships existing within our experience. To call these 'objects' (of any kind), discoverable to immediate perception (of any kind), seems to save their 'objectivity.' One may sympathize with the motive without feeling that so simple a device is fitted to its accomplishment. But Meinong promises a more epistemological study of the problem, and this may remove some of the difficulties imposed by a restricted method.

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Ueber 'Gestaltqualitäten.' By H. CORNELIUS. Zeitschrift f. Psychologie u. Physiologie d. Sinnesorgane, Bd. 22, 101.

The psychologists who have deprecated what has seemed to them an over-emphasis upon the experimental method, will welcome the tendency of the immediate present to recur to the problems of systematic and theoretical psychology. They may fail, however, to recognize that this *renaissance* of interest in the underlying conceptions of psychology is due, in great part at least, to the impetus given to all psy-

chological study by the experimental investigation of special problems a relation which is most evident when, as often happens, the psychological theorists are themselves experimentalists. This discussion of Cornelius, for example, *Ueber 'Gestaltqualitäten,'* is directly based upon the *abstractions theorie*[1] of one of the most exact of experimental investigators, G. E. Müller. This theory is summarized and supported by Cornelius, after the following fashion: Simple contents of consciousness (*Inhalten*), in spite of their 'inseparable unity,' have different modifications or attributes, in that they are similar to several 'groups' of simple conscious contents. "So, for example," Cornelius quotes from Schumann and Müller, "a simple clang may belong at one and the same time to the group of so-called deep tones, to the group of faint tones and to that of the soft tones, and one therefore distinguishes in it the three modifications of depth, faintness and softness" (p. 103). This occurrence of similarities between the contents of our consciousness is an inexplicable but fundamental fact (*Grundthatsache*) of our experience (p. 105); it is different from mere sensational consciousness (p. 106 ff); it does not consist in the mere association of such words as 'like' and 'similar' (p. 107); and it is as immediate as sensational experience itself (p. 110).

To this re-statement of the essentials of Müller's theory, Cornelius, following Ehrenfels in the main (p. 114), adds the following corollary: complex as well as simple contents belong, by virtue of their similarities, to different groups. These similarities are not merely those of the simple contents of which the complexes are made up; on the contrary, contents composed of the most diverse parts may yet resemble each other (p. 112). Such resemblances are mainly those of order and may be exemplified by likenesses of interval in melodies, and by likenesses of form in different materials. These attributes, predicated of complexes only, by reason of the similarities peculiar to them, Cornelius names *Gestaltqualitäten* or 'qualities of form' (p. 113), including under this term what is ordinarily known as 'relation' (p. 116), and carefully distinguishing the *Gestaltqualität* from the feeling which may be its accompaniment (p. 117).

Even so brief a summary has shown that the theory defended by Cornelius contains two distinct features. The first of these, the doctrine of an immediate and unanalyzable consciousness of similarity, he has, in the opinion of the writer, abundantly established, defending it successfully against the counter-theory of verbal association. But the second of his doctrines, the identification of observed 'simi-

[1] As reported by Schuman, Zeitschrift f. Psych., u. Phys., Bd. 17, 106 ff.

larity' with the 'attribute' of either simple or complex content of consciousness seems to the writer essentially untenable. The innumerable varieties of colors, brightnesses, pitches, loudnesses, which we experience, suggest the inherent unlikelihood that the one experience of similarity should be the essential feature of each. The conclusive objection to the theory, however, is reached by an appeal to introspection, which plainly contradicts the assertion that one's consciousness, say of the color-quality yellow, is nothing more nor less than one's consciousness of the likeness of one particular thing to a group of others. Certainly, this consciousness of similarity may accompany that of the yellowness, but the two are not identical.

It should be noted, in conclusion, that this doctrine of attributes does not make good its claim (p. 103, note) to reconcile the alleged simplicity of the sensation with the existence of different attributes. A content of consciousness consisting, after the manner of Müller's sensation of sound, of a consciousness of similarity to several groups, would include within itself whatever corresponds to the term 'several groups,' and would thus be far from simple.

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COLOR-VISION.

The Space-Threshold for Colors and its Dependence upon Contrast. By W. B. LANE, M.A. University of Toronto Studies, Psychological Series. 1898. Also, Transactions of the Canadian Institute, V. (2).

The question how small an object may be, and yet continue to be distinguished in its proper color, has received some attention from time to time, and notably at the hands of Woinow and of Aubert. Their work was interesting as an opening up of the subject, but it is susceptible of great improvement in the way of method and consequently of reliability of result. They made use of ordinary pigments which had not been tested for their spectral composition—a plan which is open to serious objection. For not only may a pigment which is green on the whole, reflect a large amount of spectral light on either side of the green, but a given color may be produced for consciousness in two totally different objective fashions. And not only is this true as matter of theory, but, as matter of fact, of the ordinary colored glasses of commerce, it will be found that red is red because it reflects red and absorbs blue, green and yellow, while the glasses of

other colors are more likely to get each its domination by absorbing its complementary only, and by reflecting the greater part of the rest of the spectrum—by reflecting green, for instance, and such quantities of yellow and blue as suffice to destroy each other. It is, therefore, evident that where it is inconvenient to make use of spectral colors, it is still absolutely essential to secure approximately simple spectral light, by means of reflection from and transmission through the proper combination of papers, gelatines and glasses, if results are to be obtained which are in the least capable of comparison with those of different observers, and hence, of becoming authoritative. It is equally necessary that the colored surfaces examined should be of equal brightness. The question of background is also of importance; nothing can be seen except against some kind of a background, but if the background differs in color only, the disturbances of brightness-contrast are done away with, and the question is reduced to as much simplicity as it is capable of.

The investigation before us is one of great ingenuity, painstakingness and thoroughness. Mr. Lane has succeeded in projecting a surface of any color (and of accurately measured size) against a background of any color and of absolutely equal brightness. The means employed for this is simplicity itself, and it is the same essentially as that used by Hess and Pretori (Arch. f. Opth., 1896.); it consists in a device by which the bit of colored surface and its background are illuminated by means of two different sources of light, the intensity of each of which can be regulated at pleasure, so that if one paper is darker than another it can be made equally bright by a stronger illumination. A continuous variation of size is secured by means of the Kirschmann diaphragm, which consists of two pieces of brass each in the shape of two adjoining borders of a square, and movable one upon the other by an accurately measured amount. For these experiments the diaphragm itself was covered neatly with the colored paper which was to constitute the background (which in this case therefore was actually a foreground), and through it was seen the surface of color whose space-threshold was to be determined, the important feature of the arrangement being that ground and spot could always be made of exactly equal subjective brightness. It was secured that the papers tested were of equal saturation by mixing one of them, if necessary, with gray upon the color-wheel which constituted the remoter surface.

Having secured this good apparatus, Mr. Lane naturally tested, in the case of each color against each background, including gray of the same brightness and also black [why not white?], not only the size at

which it became visible in its actual color, but also that at which it was first perceived to be of some color (though a wrong one) and that at which it was first perceived as a colorless spot. These several thresholds are referred to as the achromatic, the chromatic, and the *characteristic* color threshold. Among the results found are these: Against a black background, blue has the lowest achromatic threshold and red the highest, or, in fact, none at all, for red uniformly appeared red against black when first visible, though it passed through a brief achromatic stage against gray [as the gray was carefully made of the same brightness as the colors (p. 33–34), what was the nature of this achromatic sensation which could be distinguished from it?]. In general, the achromatic and the chromatic curves are singularly level, even where there is a colored background, a fact which it would seem tends to show the excellence of the method, but the characteristic color curves present excessive irregularities. Thus for a given observer against a black ground, the size of the opening in the two former cases varied only from one to two and three minutes of visual angle, but for getting the actual color it was necessary to have an angular opening for yellow of 37′, for blue of 20′, and for green of 15′, while red could be seen as red at an angle of 3′. (It should be noticed that as the experiments were made in a dark room, the eye must have been in a condition, very nearly, of darkness adaptation; whereas this was once supposed to be a state of extreme repose of the eye, it is now known to be a very special state of visual re-inforcement, by means of the rod-pigment, for the purpose of night-vision. Of the two sorts, the daylight space-threshold would have been very much the more interesting one to obtain.) When the background was colored (and hence when color contrast prevailed) these points were made out, for all observers: on a red ground, there was a marked lowering of the threshold for blue; on a blue ground, there was an extreme lowering of the threshold for red; on a green ground, there was also a strong favoring of red. Other than these, there were few instances of marked agreement among the different observers, except that, of course, the colors immediately adjoining, in the spectrum, the colors of the background were hard to distinguish from it. Mr. Lane lays much stress on the fact that, as he says, blue enhances the perceptibility of red, and red that of blue. But as far as red is concerned, it should be noticed that it is an extremely prominent color on black and on gray, and markedly so on green, as well as on blue. [A background of yellow was not tried; it is hard to see the reason of this omission.] It may also be that chromatic aberration (which is

extreme for red and blue) had something to do with the case; and no mention is made of the effect which it would seem might have been produced by the circumstances that the two surfaces looked at must have differed considerably from each other in respect of distance from the eye of the observer.

When the background was gray, the true-color curves of the different observers have much more uniformity throughout the spectrum; their characteristic may be stated thus, with a good deal of exactness (though Mr. Lane does not put it in just this way): the fundamental colors red, yellow and blue are much the most easily perceived; the (subjectively) mixed colors are much the most difficult to make out; green however is (like the mixed colors) a color of maximum difficulty, but this may be readily explained by the fact that in the state of darkness adaptation (which seems to have prevailed) green is very much overshadowed by the excessive functioning of the rods—the absorption of the rod-pigment, and, *pari passu* (as has been shown by König), the intensity of the darkness-vision, have each a maximum in green.

A few other points may be noted for mention. The fact that red appears first as red, and has no preceding stage in which it is seen first as gray, is spoken of as if it had not yet received explanation, or even as if it were of questionable validity. "It has *perhaps* frequently been noticed that a red light, for instance the port light of a vessel at sea or the danger light of a train, remains visible as red almost or quite as long as it is visible at all." The fact is of course well known, and is merely a consequence of the fact that the night-vision sensation is almost non-existent in red light. [Why there should be also only a very short achromatic space for the sensation produced by yellow light, in the experiments here described, is not so easy to understand.] The fovea is said to have been the portion of the retina made use of for the observation, but there seems to have been no means employed to make sure that this was constantly the case. There is hardly sufficient discussion regarding the nature of the wrong color in which each color was seen (it appears from the tables that there was nearly always such a wrong color, and that it frequently persisted until the spot had attained many times the size at which it first became colored). What appears to be most striking under this head is that red and blue were not only best perceived, but also most frequently substituted for other colors.

The fact that not only faint colors but also diminutive surfaces of color appear (unless they are red) as spots of achromatic quality before they can be distinguished (a fact first noticed by Plateau) adds

one more to the long list of disproofs of the Young-Helmholtz hypothesis. As this author says, according to that theory the composite colors, as orange, olive, purple, peacock (blue-green), when their retinal image is very small, should have a tendency to appear in one or other of their component colors, on account of the isolation of the nervous elements stimulated by so small an exposure of colored surface. "But the contrary is the case, as we have seen. Instead of vanishing into some other simpler element, the alleged composite becomes a colorless point of light, a phenomenon which this component theory fails to explain." The primary colors, also, ought not to have an achromatic sub-threshold; "for if these color-tones are the constituents of white light, we should never expect to find, as is the case, that they lose their color at small angular sizes, and appear as that white light which is supposed to be the product of all three." This argument has, of course, been already made much of, and in the first place in the opposite sense, for Holmgren thought that a white spot did become, when very small, one or other of the different fundamental colors, as it fell upon one or another of the visual elements. But the inversion of the fact, brought about immediately afterwards, both by Hering and by König, caused the argument, of course, to work strongly in the other direction.

The careful reader of this paper will come upon certain discrepancies of statement which are without doubt due to misprints of some kind, only it is sometimes difficult to know which of two statements is the correct one. For instance, it is said (Table XVIII.) that orange or blue is seen as red up to the whole possible size of the opening (*i. e.*, 2° 7′), and also that it is seen, by the same observer, in its true color at 11′ 40″; that it remains red to the end for another observer, and is also seen as orange at 1° 54′; that eight observers saw it, in the average, in its true color at so small an angle as 30′ 40″, and also (p. 53) it "has scarcely any characteristic space-threshold within the limits of the diaphragm aperture," and "is never seen as orange except by one observer"; again, "orange remains red and not orange up to the fullest opening of the diaphragm." "This notable phenomenon is therefore not a trifling irregularity of a limited retinal area, but is more probably a property of color phenomena which must be recognized and reckoned with in any adequate theory of colors," but which is the phenomenon? (The curve drawn for the eight observers agrees with the table and not with the text.)

The strong tendency which red and blue show towards re-inforcing each other is brought by Mr. Lane into connection with a case of

color-blindness described by Kirschmann (Phil. Stud. VIII., 199), monocular and congenital, in which red and blue were the colors which could be seen. Mr. Lane's paper is followed by a description of a new case of apparently the same nature, though that was not so certain, as the defect was not monocular. Similar cases have been described by König; they are not easy to understand, but they may possibly be due to a defect in some higher region of the brain, where there are undoubtedly different centers for the different color sensations.

These results are quite without connection with those of Aubert, as given by Helmholtz (Physiol. Optik, p. 375); in view of the importance of the subject, and the difficulty of attaching any meaning to the facts as here given, it is very desirable that they should be confirmed by other observers. This does not mean that the present paper does not bear every mark of being a thorough and exact piece of research.

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MEMORY.

Untersuchungen über das Gedächtniss für räumliche Distanzen des Gesichtssinnes. Von ZWETAN RADOSLAWOW-HADJI-DENKOW. Philosophische Studien, XV., 3.

The object of this work is the investigation of memory of short distances.

The apparatus consisted of two dark points on a white background so arranged that one might be quickly moved to or from the other by means of a micrometer screw.

The methods employed have long been in use.

1. Herr Denkow finds that as the time between the observation of the distance to be remembered and our attempt to remember it is increased the accuracy of recollection is decreased. In other words, the longer the time through which we have to remember a distance the greater the inaccuracy of the memory.

The formula in which this conclusion is expressed amounts to an assertion that the accuracy of memory for distance is within limits practically proportional to the logarithm of the time.

This is hardly more than a confirmation of the results of Ebbinghaus, Wolfe and others and was to have been expected. The only case known to me in which it has been doubted that this law is invariably true is that of Paneth's[1] remarkable experiments on memory of

[1] Paneth, *Centralblatt f. Physiologie*, 1890, 81.

time intervals. Here, apparently, the memory is as accurate after a lapse of five minutes as after three seconds.

2. It was discovered that memory for distance is considerably more accurate after a lapse of 2½ seconds than after 1 second. Wolfe, Lewy and Bigham met with this same phenomenon, which, indeed, appears to be universal.

Herr Denkow attributes this strange fact neither to fluctuations of attention nor to the nature of memory as such, but rather to external circumstances, such as haste, confusion, inability to 'pull one's self together' in so short a time as one second, etc. In this he agrees with Lewy.

Whatever its explanation may be, the phenomenon is one of great importance to experimental psychology and has often been overlooked in situations where it must have been of consequence. For instance, we find that in many attempts to investigate the relationship of Weber's law to sound, the experimenters have allowed the stimuli, which, of course, are necessarily successive, to follow each other at an interval of only one second. Evidently better results would have been attained had the interval been lengthened to two or three seconds.

3. In the curves expressing the relationship of the time interval to the accuracy of recollection for distance there are two great variations, the first appearing usually at the ten- and the second at the thirty-second point. The memory of distance is more accurate after a lapse of 10 than after a lapse of 7 seconds, and also more accurate after 30 than after 25.

Baldwin, Bigham and Lewy have also noticed this phenomenon. Herr Denkow does not know how to explain it. However, he feels certain, and apparently with justice, that it cannot be reduced to accident or to external conditions, but that it must be a phenomenon of the memory function itself.

4. The experiments were carried out in the following manner: First the distance to be remembered was exposed to the view of the subject, then hidden, then after the desired lapse of time another distance was brought to sight. Finally the subject was required to determine whether the second (comparative) distance was greater than, equal to or less than the first (normal) distance. Now, it appears all through the experiments that in those instances in which the comparative distance was actually equal to the normal one, it was in the majority of cases judged to be greater. That is to say, there was an evident and constant tendency on the part of the subject to judge the second distance as greater than the first, when, as a matter of fact,

they were equal. This illusion must be the result, either of an undervaluation of the first or an exaggeration of the second distance. Several investigators who have elsewhere observed the same phenomenon have naturally assumed the former case to be the true one. But our author takes the opposite view, and speaks of the illusion as one of the overvaluation of the comparative distance. In explanation he offers the following ingenious hypothesis: "Upon shutting the lids the eyes are turned downward and toward each other; but upon opening they take an upward and outward direction (*i. e.*, they diverge). It therefore seems to me probable that the subjects of the experiments, who in the interval between the first exposure and the second kept their eyes closed, judged the comparative distance to be greater than it actually is because of the feeling of increased tension which accompanies the motions of looking upward at the objects."

In their experiments on 'memory for square size' Baldwin, Warren and Shaw, working together, came to a conclusion which seems flatly to contradict the existence of the phenomenon which Herr Denkow is here trying to explain, for they claim to have discovered that when the comparative and normal figures are actually equal in size there is a tendency to judge the former as the smaller. Thus instead of speaking with Herr Denkow of an 'exaggeration of the comparative distance' they refer to the direct opposite, *i. e.*, 'an increase in the memory image.'

Herr Denkow takes the work of these investigators into account, but endeavors either to show that in spite of their own assertions the results really confirm his position, or to cast doubt on the value of their work. It seems to me that he has failed in both attempts. The situation is certainly an interesting one and worthy of further research.

In this connection it would have been well had our author extended his reading a little further. Tschisch,[1] Lehmann,[2] Merkel[3] and Starke[4] found that when two sounds objectively equal in intensity are given, one after the other, the second is judged to be the greater. Moreover, Leuba,[5] working on the classification of artificial stars of different intensities, found that a strong light stimulus tends to be remembered as fainter than it really was, while, on the other hand, a weak one tends to be remembered as brighter than it actually was. In

[1] Ueber das Gedächtniss f. Sinneswahrnehmungen. Internationaler Congress f. Psychologie in München, 1896, S. 95.

[2] *Philosophische Studien*, VII., 169.

[3] *Philosophische Studien*, IV., 137.

[4] *Philosophische Studien*, III., 270.

[5] *American Journal of Psychology*, V., 370.

order to explain this he suggests the general theory that consciousness tends to form for itself a sort of single and typical representative of each class of experiences; that this is, as it were, a residuum of previous perceptions; that it corresponds approximately to the middle term in the scale of perceptions; and finally that our memory images are, so to speak, attracted toward this mean and typical term. Thus intense, large, or temporarily long perceptions would have a tendency to be undervalued in memory, and *vice versa*.

It would have been interesting to have had Herr Denkow's opinion of these investigations. It certainly seems strange that such a valuable and pertinent piece of work as that of Leuba should be overlooked. This, however, is unfortunately a characteristic trait of work in this sphere. I cannot now remember ever having seen Leuba's article referred to or quoted by any writer on these subjects, and yet there is scarcely anything in this line which equals it in suggestiveness.

5. In most of the experiments the subject was instructed to keep his eyes closed during the interval between the exposure of the normal and comparative stimuli and was otherwise left undisturbed. However, an effort was made to investigate the effect which the deflection of attention during the interval might have on the accuracy of recollection. In order to determine this the attention of the subject during the period between the two stimuli was occupied in various ways: either by sounds (metronome strokes), impressions of sight (colored surfaces) or by reading aloud to him.

Our author comes to the important and astonishing conclusion that in all cases the accuracy of memory is enhanced if during the interval the attention is deflected from the thing to be remembered to something else.

The explanation offered is based upon the phenomenon of fatigue.

The points of interest which I have tried to present do not exhaust the work, which contains much of importance.

The researches are carefully carried out and are no doubt the product of much labor. They will certainly be of value to the future student of memory. Indeed, they should take rank with the best efforts in this sphere. It is only unfortunate that they cannot be carried further. We have as yet no work of this sort which has nearly approached completion, and this article, like the others, comes to an end just when one feels that he is about to get some real insight into the functions under consideration. Both Wolfe and Herr Denkow were students in Wundt's laboratory. Each has worked with his own material and yet their investigations have in a sense overlapped. The re-

searches of the former made it almost certain that three of the five points mentioned above would be observed by the latter. What is needed in the experimental study of memory is not so much the examination of material still unexamined as the patient working to the end of investigations already begun.

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RHYTHM.

Zur Grundlegung einer Aesthetik des Rhythmus. DR. MAX ETTLINGER. Zeitschrift für Psychologie u. s. w., Vol. 22 (1900), No. 3.

Dr. Ettlinger's monograph is a theoretical discussion of the nature of the rhythm experience, based chiefly upon the published work of Meumann and Bolton. The introduction sets forth the problem presented by an investigation of rhythm. Rhythm does not exist as a pure art-form. It is a particular æsthetic aspect of music, dancing and poetry. Its appreciation is a form of time- and intensity-perception, specialized only by æsthetic worth. Its objective relations may present chronometrically simple forms and the periodicity of its groupings may conduce to a clearer apprehension of the successive elements, but the rhythm experience does not consist in any such factors; it lies in the connection of a directly conscious process of feeling with a simultaneous series of ideas. The value of this subjective factor is evident. Rhythm is not presented in an objective form like architecture; it must be produced afresh on each occasion. It is necessary, then, in addition to an investigation of the constitution of the rhythmic art-forms to consider our relation to them when we take pleasure in them. We must determine the factors of the rhythm experience. This experience arises under two roughly discriminable conditions—first, when it is connected with a periodically changing perception content, and secondly, when it consists in a wholly subjective rhythmization. In the latter form we seem to approach most closely to an isolation of the peculiarly subjective elements. The paper is therefore divided into three sections: first, the phenomenon of subjective rhythmization; second, the objective factors of rhythm, and third, the rhythmic art-forms and their pleasantness.

Subjective rhythmization occurs only when the successive elements are not specially attended to, and when the members of each adjacent

pair lie within the same act of immediate time-perception, for these successive elements are not independent experiences associated in memory, but are essentially one experience, though their unity lies only in a subjective synthesis. Among the facts to be accounted for by any theory of such rhythmization are the following: The sensation with which one is specially concerned appears louder than the surrounding sensations. The louder (subjectively) tone seems also the longer. The pause following the accented element appears shorter than that following the unaccented. And the series of sounds must be listened to for a certain time before subjective rhythmization arises. The author bases his own explanation upon the appearance of two tendencies accompanying the hearing of any such series. In the first case—when the elements are attended to singly—the gaze is directed backward, each sensation is assimilated with the foregoing, and the new comes unexpectedly. In the second—in which the individual elements are overlooked—the series is apprehended as a constant forward movement, in which each element forms a point of transition to the next. There are here, then, two opposing forces, the one forward-striving and positive, and the other negative and retarding. The basis of the positive tendency lies in the close temporal connection of the whole series; the basis of the negative lies in the isolation and force of the individual sensation. The inner accent of subjective rhythm is nothing else than the emergence of the secondary retarding tendency, while in the unaccented elements the primary tendency to forward movement prevails. Through the balance of these two forces accented and unaccented are united in one organic unity, the rhythmic group.

Of the various objective factors of rhythm the chief are relative intensity, duration and form of succession in the elements. A fourth, the variation in quality among the elements, though of the greatest importance in music, lies here in the background, because of the high secondary significance of the symbols. Two closely succeeding sounds differing in one of these directions at once compose a unity which does not appear if they are uniform. In the latter case the feeling of rhythm arises only gradually, while in the former it appears at once. This process of unification consists in the appearance of an image of motion, a motion opposed, transcending opposition, and freely reinstated. The retarding factor is given in the accented element, the forwarding in the unaccented.

When a variation is introduced in any one of these directions, it is subjectively contributed in other directions. The most common of these is the change in intensity, and next that in duration. These ob-

jective factors are not of equal value. Those are the most powerful which have the greatest retarding influence, namely, intensity and pitch. No single form of change is indispensable; the rhythm experience may connect with any one. In one regard, however, the conditions are fixed: the form of grouping is always determined by duration, whether the intensive accent agrees with it or not. This does not agree with the reviewer's experimental results. Modes of arrangement obtained by specific changes in the duration of the successive intervals were readily dissolved, and inverted forms obtained by increasing the intensity of the accented element.

In his discussion of rhythmic art-forms the writer confines himself to a consideration of modern German verse-rhythms. Rhythmic synthesis does not cease with the simple group. Beyond this there is an oscillating play of opposing forces sustained throughout long periods and possessing a higher integration than that of any individual group. This form can be understood only from a consideration of the whole series, never from that of single groups. It presents a system rounded out to completeness, with beginning, climax and close.

Its simplest units present two phases, first that of the falling group $\hat{d}\,d$, in which the preponderance of the retarding tendency marks the passage from rest to motion, and the rising group $d\,\hat{d}$, presenting a passage from gentle motion to a decisive stop. Now since the determining and clarifying of the rhythmic form depend upon the stress, the falling group is fitted to initiate a movement but not to close it. The rising group, on the contrary, gives no clear beginning to a movement, but, placed at the end, brings it definitely to a close. Also since the accent directly affects the apparent length of the interval following it, the *tempo* of the falling group is retarded, that of the rising group accelerated. This the Greeks recognized by calling the trochee *hesychiastic*, 'restful,' the iambus *diastaltic*, 'stirring.' Hence also Bach's usage of the former in the *choral*, and of the latter in the *gavotte*.

Dactyl and anapæst are explicable according to the same principle as the trochee and iambus. The anapæst leads from rest through motion to a stop more decisive than that of iambus. It shortens the succeeding pause still more, and is still more hasty. The harshness of its sudden close is softened by the mediation of the next initial element. It is, therefore, not suited to conclude a measure. (This is not the case in English verse, where the typical anapæst closes with the final accent unmodified, though it is questionable if this be not a less perfect art-form than the German.) The dactyl passes from rest

through motion to a pause, and has an effect like that of the trochee. It is fitted to introduce a restful movement but not to close a line. The Greeks regularly observed this limitation in the function of the secondary falling group by closing their dactyllic lines with the trochaic measure. Dr. Ettlinger's article is a welcome contribution to the study of rhythm.

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A Study of Lapses. By H. HEATH BAWDEN, A.M. Monograph Supplements of the Psychological Review, Vol. III., No. 4, April, 1900.

This is a study of certain minor forms of aberration in thought which find their way into spoken and written language. These errors are what are commonly called lapsus linguæ and lapsus calami. The data here studied are chiefly collated from ordinary speech and writing. Some use is made of experimental material, but this for the most part is reserved for further consideration in a 'Genetic Study of Lapses.' The lapse is considered first from the standpoint of its conditions and the general problems connected therewith. Next follows an analysis of the factors which enter into the speech and writing consciousness. Then we have an interpretation of the lapse as an example of mental assimilation. And lastly there is a statement of the laws or principles involved in the occurrence of the lapse. A number of subordinate questions come up for discussion, such as the question concerning the psychological unit in the use of language, the problem of the fundamental imagery of 'meaning,' and the criticism of certain current conceptions, such as the use of the terms 'sensory' and 'motor' in psychology and the so-called 'laws of association.'

The lapse is found to be a subconscious formation, the product of some process beneath the threshold. That is, these errors are purely ideomotor in origin. There is no hint of what sort of an error will occur until it is actually seen or heard after it has been made. The first experience of the person himself is the auditory or visual perception of a lapse, *i. e.*, a word is recognized as having been spoken or written in a connection where it has no meaning or where it gives a meaning that was not intended.

Lapses are described as belonging to the abnormal or pathological phenomena of mental life. The fact is, that lapses run right over into paraphasia. The only difference is that in the aphasic patient the

condition becomes permanent, while in the normal experience it is but transitory. Intervening stages are found, however, which make it clear that the conditions are the same in kind for the two sets of phenomena.

On the surface, lapses appear very much akin to what are ordinarily called sense illusions. We often say and write just such peculiar things as we hear and see. Sometimes it seems as though the lapse were merely an exteriorized illusion in the interpretation of verbal symbols. Upon careful analysis we find the one passing over into the others, or rather the two types of error at certain points are indistinguishable. When the sensory elements preponderate we call the error a sense illusion; when the motor elements preponderate we call the error a lapse.

In its unique and bizarre forms the lapse reveals the real complexity of our verbal consciousness. It may be regarded as an instance of what Stout has called coalescence or overlapping, which presupposes the existence of certain mental systems which come into conflict or competition and under varying conditions exhibit these peculiar phenomena of coalition or fusion. Such verbal coalescence involves the interconnections of at least three types of imagery, the auditory, the visual and the kinæsthetic. Speech is chiefly in auditory-kinæsthetic imagery; writing in visual-kinæsthetic imagery.

These different types of imagery combine in different ways, according to the past experience of the individual, into more or less flexible units of meaning.. Even a lapse represents the tendency of the mind to make something with sense out of the impressions which it receives from without; it represents the attempt to find something or to put something into the bare symbols. Lapses show that the setting or context of a letter or word is what determines its meaning, for sometimes the order of words in an entire phrase will be inverted or transposed and yet the process of expression be not in the least interrupted because the unit of meaning in that case happened to be larger than the merely verbal error. This shows that speaking and reading at least are meaning-wise rather than spelling-wise. Lapses show very plainly that the psychological unit of language is very different from the merely grammatical or formal unit, and much is to be expected from a comparison of these phenomena with the results of philological research.

This view of the nature of the psychological unit in verbal consciousness means that the old static conceptions of laws of association must be revised or at least restated. The so-called laws of similarity

and contiguity, for example, in the light of such phenomena as lapses, are to be stated as a single law, that contiguous similars tend to coalesce. But we must seek further than these so-called laws for an adequate functional statement of the meaning of these errors. This is found in the two general principles of habit and attention—represented, in the case of lapses, by the persistent and anticipative forms. Persistence represents psychologically the standstill of the organic adjustment involved in speaking and writing. So long as you are doing over and over again the same thing that you have always done you are not getting ahead any in your experience, in this case, in your process of verbal expression. You are simply holding your own: in one sense, you are going backward, since everything else about you may be going forward. Anticipation, on the other hand, is the psychical correlate or psychological statement of organic adjustment and readjustment. It is what otherwise we call attention. Attention is selective, and this selection involves anticipation. Attention goes to or is developed at or in the point of difficult adjustment. Ease of adjustment, ready adaptation, mean reduction of consciousness and thus of attention, of anticipation. Conscious experience is thus a constant disturbance of a tendency toward equilibrium between the attentional and the automatic processes. The errors or lapses appear at the point of readjustment, at the point of tension between these two processes. They appear with the breaking up of an habitual process and in the opening up of a new form of expression in speaking or writing.

A brief concluding section is devoted to suggestions for a psychology of the ludicrous. Lapses suggest that the essence of the sense of the ludicrous consists in the consciousness of some deviation from the common or normal in experience, and that it has a fundamentally ethical significance.

THE AUTHOR.

Minor Studies from the Psychological Laboratory of Cornell University, XIX. Visual Reading, a Study in Mental Imagery. By W. B. SECOR. Am. Jour. Psy., XI., 2,—Jan., 1900. Pp. 225–36.

The author investigates the question, "Is it possible to read without mentally hearing the words, and without feeling them in the throat, the mouth or on the lips?" He concludes that it is; that "the auditory element is a much more persistent factor," "that articulation and audition are to be regarded as aids in reading," and that "the prominence of the auditory and articulatory elements in reading depends

upon the type of the individual and upon the amount and kind of reading done by him." He experimented upon four subjects in the following ways: First, he determined the 'type' of his subject by getting him to introspect upon words with sense imagery content, such as *drift-wood*, *waves*, *violet*, *book*, etc. After the subjects had been classified as chiefly visual or auditory, they were given passages to read, certain inhibitory actions being exacted of them. Thus articulation, incipient or imagined, was inhibited by the subject's whistling, reading aloud a different thing or pronouncing a letter, while auditory imagery was driven away by various noises endured by the subject (such as xylophone playing). The result was 'reading by vision alone,' in the case of three out of four subjects. The fourth subject, like the present writer, was unable to banish the auditory imagery. The result of rapid reading with the auditory-minded person seems to be that here and there a word is imaged, but as if slurred, and the rest is a blur—both of sound and of ideas. Unfortunately Mr. Secor does not give any indication of the nature of reading by vision alone, or its value. The present writer not only finds that reading without mental audition is almost impossible and quite meaningless, but he has always failed to get any notion of what may be the state of mind during silent reading of certain persons who say that they read twice or thrice the ordinary rate of possible articulation, say 200 to 300 pages an hour.

WILFRID LAY.

Ueber Ablenkbarkeit und Gewöhnungsfähigkeit. By RAGNAR VOGT. Psychologische Arbeiten. III Bd., I Heft, 1899. Pp. 62–201.

The main interest of the investigation under consideration centers about the methods employed by Vogt in his studies on the diversion of attention and the formation of habits, both habits of actual work and habits of resistance to distracting circumstances. A person is designated as "ablenkbar, wenn seine Aufmerksamkeit durch Eindrücke der Aussenwelt oder durch emportauchende Vorstellungen leicht in eine neue Richtung gedrängt wird und damit ihre selbständige Festigkeit verliert."

In order to test the degree of distraction, various mental tasks are imposed, and into these tasks different diverting circumstances are injected. As a measure of the degree of distraction thus involved, Vogt takes into consideration the diminution entailed in both the quantity and quality of the work accomplished under the varying conditions. But, as is to be expected, a person soon becomes accustomed to dis-

tractions. These latter are, in fact, merely parallel events accompanying the main event. The degree of habituation, or practice curve, is thus determined by a comparison of the successive improvements in both the quantity and quality of work accomplished under similar circumstances during a series of experiments.

During by far the greater part of the investigation Vogt functioned as his own subject, 130 out of 186 hours of investigation falling to his lot. A few experiments were also conducted on three other persons and on two defectives.

The entire investigation may be divided into two main groups : I. Experiments accompanied with discontinuous interruptions, and II. Experiments accompanied with continuous interruptions. The former series consisted of three subgroups : (A) *Auffassungsversuche*, (B) *Additionsversuche* and (C) *Lernversuche*. Each of these subgroups will be briefly described.

(A) Auffassungsversuche. These experiments consisted in part in the perception and naming of various series of 331 nonsense syllables pasted on a rotating kymograph cylinder and seen through a small slit. Each experiment consumed about six minutes, with an average of four minutes rest between the consecutive members of the series. To determine the influence of diverting circumstances, Vogt introduced the marking of time by moving a finger in response to metronome bell-strokes at the rate of 19 per minute, while the syllables were being read off. The distraction thus introduced is a simple reaction. As a complication of this reaction, and in order to introduce a definite memory factor in the parallel event, a similar series of experiments was performed wherein the same reaction occurred with the additional element of a double finger reaction on every fourth beat. The remainder of these *Affassungsversuchen* consisted in the discrimination of the letters l, n, and s occurring in an unintelligible text (Finnish), under generally similar diverting circumstances as before, and accompanied both with and without the cancellation of these letters.

(B) The *Additionsversuche* consist of two main species: (*a*) The continuous addition for 10 minutes of columns of figures up to 100, carrying the *endziffer* of the sum to the next column and thus continuing to the time limit, when a rest period of 5 minutes was interpolated; (*b*) The addition of successive pairs of figures arranged in columns, with the writing down of the successive sums. Time limits as above. The same single and double finger reactions in response to metronome bell-strokes were introduced in this series as were

employed in the first sub-group. In order to vary the conditions of experiment and bring to light different factors conducing to distraction of the attention, other and more complicated reactions were at different times introduced in the course of the addition experiments; for example (1) reacting on each metronome bell-stroke by making a dot; (2) reacting as in (1), with the further condition of drawing a cross at each fourth bell-stroke; (3) reacting only at each fourth bell-stroke by drawing a cross.

(C) *Lernversuche.* The main task of this series consisted in committing to memory twelve-place numbers and nonsense syllables, based on Oehrn's method. The parallel diverting events employed were, with a few omissions, similar to those used in the later experiments of the preceding subgroup.

An appreciable time elapses between successive beats of the metronome. For this reason the distracting influence is more or less sensibly discontinuous. There is a tendency in the addition experiments, for example, to postpone reacting to a parallel event sufficiently long enough so as to admit of fulfilling portions of the main task within definite periods and then injecting the parallel reaction at the following intervals. In order to determine the nature and the method of the influence exercised by continuous distractions of the attention on certain mental processes, and by various parallel processes on each other, Vogt carried on a second series of experiments to which allusion has already been made under the caption II. *Versuche mit ununterbrochenen Störungen.* Within this group occur four main subgroups, designated respectively as: (A) *Hersagen von Gedichten bei fortschreitenden und zifferweisen Addition;* (B) *Hersagen von Gedichten beim Zahlenlernen;* (C) *Hersagen eines Gedichtes beim Niederschreiben eines andern Gedichtes;* (D) *Verbindung von Zählen mit niederschrieben des Alphabets.* In this second group of experiments the reactions employed in the first group are not used. As may be noted, the parallel tasks imposed furnish a sufficient amount of distraction and counteraction for the determination of the problem in hand.

The headings of the various subgroups are sufficiently suggestive to indicate the general nature of the experiments involved within each class. The experiments of subgroup (A) consisted of the recitation of memorized selections accompanied with the two species of addition experiments already described. Subgroup (B) consisted of experiments wherein the memorizing of twelve-place numbers was correlated with similar recitations of memorized selections. Subgroup (C) in-

volved the recitation of memorized selections both loudly and softly, while at the same time other selections were reproduced in writing. In subgroup (D) numbers were counted backwards from 200, in combination with the synchronous writing of the alphabet. Vogt does not indicate that any rest-periods were interpolated in the experiments of the first series (A), which as a rule continued 1¼ hours. The experiments of subgroup (B) consumed 5 fifteen minute periods with intervals of 10 minutes rest between the periods. Series (C) and (D) consisted of from 4 to 6 five minute periods with rest intervals of 5 minutes intervening.

In addition to these main lines of investigation, Vogt instituted another interesting series of experiments. He employed the well-known method of cancelling certain letters of a printed text as the main task. The texts used were in the vernacular and in Finnish. No extraneous diversions were introduced. The diverting elements consisted in the more or less conscious associations encountered while cancelling letters in the German text. It is worth recording in this place that the intelligible text exercised a very appreciable diverting influence on the main task undertaken. A few experiments of a simple nature were performed on two defectives, a paralytic and an epileptic. Furthermore, Vogt subjected himself to a series of experiments with the purpose of ascertaining the influence of alcohol on some of the problems under investigation.

The above synopsis indicates in brief the nature and manner of the experimental methods employed. As the result of his investigations Vogt draws a number of interesting conclusions clearly stated and serially arranged. Some of these conclusions experimentally substantiate our common knowledge concerning the play of factors in diverting the attention and the formation of habits. Others offer interesting problems for further research. Of the more important conclusions the following affords a summary:

(1) The simpler mental activities of apprehension and perception are less disturbed by distracting circumstances than are the more complex activities involving memory and association.

(2) Mental tasks which do not offer opportunities for either complete or partial relaxation are much more influenced by the operation of rhythmic disturbances than are tasks which are more or less discontinuous. The reason for this is to be found in the fact that discontinuous operations (*e. g.*, *zifferweise addition* and the like) contain empty periods into which there is a tendency to inject the parallel event.

(3) Two synchronously occurring tasks in which the same essential psychical processes are involved exercise a very strong disturbing effect on each other. Such tasks can only be successfully performed together when it is possible to effect a change in the manner of doing one of them. For example, a person who in adding, counting, and such like, is a motile, will best be able to perform these operations as parallel events to the main task of reciting a memorized selection, when he has accustomed himself to the transference of the former to the visual field, thus allowing the main work to monopolize the motor consciousness, and in particular the articulatory movements.

(4) Two pieces of work, one of which entails articulatory movements while the other is concerned with writing movements, can easily occur together without causing any great counteracting disturbances.

Practically all of the experiments of this investigation offer rich opportunities for the formation of habits, both habits of actual working ability and habits of resistance to distraction of the attention. As might be expected, the gain in power is relatively the greatest during the first few days. Relative to the persistence of a habit, one interesting case is worth recording. During the course of the *Additionsversuchen* Vogt had occasion to allow a one week's pause to elapse, and immediately thereafter a four months' pause. As a result, he notes essentially the same loss of facility in each case. This phenomenon, he writes, agrees with some of Ebbinghaus's experiments on the value of the time factor in memory.

In conclusion, it remains to state that the experiments of this investigation are judiciously planned and admirably and carefully executed. A healthy, conservative tone tempers the entire work. Vogt frankly recognizes some of the limitations of his total results. Yet the methods employed apparently harmonize with the problems in hand, and offer suggestive bases of departure for work along similar lines. Individual differences must be accounted for; hence, in order to gain valid general conclusions relative to the problems under consideration, the conditions of an experiment must vary sufficiently so as to accord proper recognition to the visual, auditory, tactile, and motor type-forms. In Vogt's opinion, "Eine Verbindung von zwei Gleichzeitigen Reactionsbewegungen, die nicht dieselben seelischen Hülfsmittel in Anspruch nehmen, würde vielleicht am ehesten die Bedingungen der gleichmässigen Arbeitsweise und der Störungsempfindlichkeit vereinigen."

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NEUROLOGICAL.

Sur quelques particularités de structure des cellules nerveuses. By S. C. MARTINOTTI. Archives Italiennes de Biologie, XXXII., Fasc. II. 15 December, 1899. Pp. 293–308.

The research summarized in this article has probably added something to the histology of the nerve-cell and consequently to our knowledge of its physiology. Not only from his own reputation as a competent observer, but because Dr. Martinotti corroborates the opinions of the renowned Golgi, are these new propositions worthy of at least present acceptance.

The observations reported were made on the cells of the cerebral and cerebellar cortices and on those of the spinal ganglia, apparently about all of the newest and best staining methods being employed.

The cell-bodies have to the author a hollow appearance, as have also the 'protoplasmic prolongations,' and this hollowness can be seen even in the 'last ramifications.' One sees in the periphery of the cells a multitude of juxtaposed fibrils which sometimes run parallel to each other, and sometimes cross and intersect and anastomose until they almost constitute a very fine-meshed and delicate membrane. These fibrils vary greatly in their modes of anastomosis and in their size, being sometimes thick but often very fine; their course is not always straight, but at times is tortuous. Some fibrils extend downward into the mass of the cell instead of merely about its periphery.

This transparent membrane, thus constituted in part, seems to enclose not only the cell-bodies proper, but to extend out around all the protoplasmic prolongations and even about the axis-cylinders arising in the cell. Chemical analysis and its physical properties both go to show that this membrane is *neurokeratinic* in composition, tough and transparent and very perfect in its osmotic conductivity while at the same time quite non-conductive of any nervous current. Such a membrane, then, is precisely what the nerve-cell would require to support firmly its semi-fluid protoplasmic substance and that of the prolongations. At the same time the membrane serves as a perfect means of isolation, being complete except at the tips of the teleodendria, where two cells communicate both physiologically and at times structurally with each other. The protoplasmic prolongations are undoubtedly, thinks Dr. Martinotti, for the purpose of furnishing a larger surface-area, so that, exposed to the surrounding nutritive plasma, the cell may be better supplied with food. [One sees a similar arrangement for the same purpose in the villi of the small intestine.]

Such an hypothesis, to use a conservative term, is right in line with recent observations on other sorts of cells, as for example those of which muscle is composed.

Ueber die Lage des motorischer Rindcentren des Menschen nach Ergebnissen faradischer Reizung derselben bei Gehirnoperation. Archiv für Anatomie und Physiologie, 1899, Supplementary Volume, second half, 25 October, 1899. Pp. 543–546.

Ueber die sensiblen Functionen der sog. motorischen Rinderzon des Menschen. By W. v. BECHTEREW. Ibid., 1900, Nos. I. and II., 31 January, 1900. Pp. 22–24.

These two very recent articles by the same author are well considered together. Both are brief but concentrated reports on the present status of our knowledge concerning the cortical sensori-motor centers.

In the former of the articles are summarized the facts derived from three surgical cases of trephining for epilepsy performed by other surgeons in the course of which Professor Bechterew stimulated with an induction current the exposed 'motor' region of the cortex. Besides these cases ten others of like nature are involved in the conclusions, four conducted by Ferrier and six by Horsley, in like manner. Two of the present investigator's subjects were boys, aged respectively eleven and sixteen years, while the other was a man. His conclusions as to the motor centers may be condensed to the following statements:

The general arrangement in man is precisely as it has often been found to be in apes. The centers for the lower extremity are located in the upper part of the gyrus centralis posterior. The centers for the upper extremity are in the middle part of both central convolutions. Immediately under the middle of these centers lie those for the thumb and the remaining fingers. The facial centers lie in the under part of the central convolutions. The parts controlling the lateral movements of the head and the movements of the eyes are situated in the posterior portion of the second frontal convolution and about there. The centers for the muscles of the back are found to be on the surface of the anterior central convolution in the upper half of the center for the upper extremity (in apes they are on the upper surface of the lateral lobes in the upper portion of the gyrus centralis anterior). In men as in apes there is a different center for the movement of the thumb and for that of the remaining fingers, and this lies in the central convolution under or near the centers for the upper extremity.

In the second article direct physiological and anatomical evidence

is adduced for facts apparent to psychologists for some time. Many operations on apes and recently the above-mentioned trephining therapeutic operations on persons with severe epilepsy have given quite harmonious results. In one of Horsley's cases there was found a disturbing cause of irritation in the center for the thumb above defined. The motor paralysis was in this instance accompanied by anæsthesia to light touch-stimuli; difficulty in localizing pressure applied to the thumb; numbness and a sensation of cold; with an inability of locating the thumb in space. The intensity of such psychical disturbances seems to be proportional to the severity of the occasioning material injury in the cortex. Muscular sensation is destroyed whenever motivity of a part is abolished. All three of Professor Bechterew's subjects above mentioned had sensory disturbances similar to those of the paretic case already described.

"These discoveries," says the investigator in concluding the brief article, "set aside all doubt that the dermal sensibility and the muscular feeling in man are located in the known cortical centers together with voluntary movements, as experimental discoveries in apes have shown. This fact will be intelligible when we recall that up to a certain point the two known qualities of sensibility are determined for the voluntary movements," namely, the sensations of movement in the skin and the muscular 'feelings' themselves.

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NEW BOOKS.

The Grammar of Science. KARL PEARSON. London, Adams & Charles Black; New York, The Macmillan Company. 1900. Second edition, revised and enlarged. Pp. XVIII + 548.

The Spiritual Life. Studies in the Science of Religion. GEORGE A. COE. New York, Eaton & Mains; Cincinnati, Curts & Jennings. 1900. Pp. 279.

La philosophie naturelle. W. NICATE. Paris, V. Giard & Brière. 1900. Pp. XI + 308.

Les lois sociale devant le droit naturel. F. DUGAST. Paris, V. Giard & E. Brière. 1900. Pp. 61.

National Educational Association, Journal of Proceedings and Addresses of the Thirty-eighth Annual Meeting held at Los Angeles, Cal., July 11-14, 1899. Pp. 1258.

La constitution du monde. Dynamique des atomes. Mme. C. ROYER. Paris, Schleicher Fr. 1900. Pp. XXII + 800.

La France au point de vue moral. A. FOUILLÉE. Paris, Alcan, 1900. Pp. 416.

Rechts- und Linkshandigkeit. F. LUEDDECKEUS. Leipzig, Engelmann. 1900. Pp. 82.

L'évolution du droit et la conscience sociale. L. TANON. Paris, Alcan, 1900. Pp. 166.

NOTES.

A NUMBER of teachers and students of Philosophy in the West met at Kansas City on January 1, 1900, and organized the Western Philosophical Association. The aim of this society is 'to stimulate an interest in philosophy in all its branches and to encourage original investigation.' All advanced students of philosophy are eligible for membership. There are about thirty charter members, representing the States of Missouri, Nebraska, Kansas, Colorado, Iowa, Minnesota and South Dakota. The officers of the Association for the year 1900 are: Frank Thilly, Ph.D., University of Missouri, President; Olin Templin, A.M., University of Kansas, Vice President; A. Ross Hill, Ph.D., University of Nebraska, Secretary-Treasurer; with G. T. W. Patrick, Ph.D., University of Iowa, and Cleland B. McAfee, Ph.D., Park College, as additional members of the Executive Committee. The first regular meeting of the Association will be held at Lincoln, Nebraska, during the Christmas holidays of 1900.

PROFESSOR WILLIAM JAMES, Professor Wilhelm Wundt and Professor Max Heinze were elected corresponding members of the Berlin Academy of Sciences on the occasion of the celebration of its 200th anniversary.

DR. EDWARD CAIRD, Master of Balliol College, Oxford, who was formerly Professor of Moral Philosophy at Glasgow University, has been nominated as Gifford Lecturer in the latter University, in succession to Sir Michael Foster, M.P.

DR. SIMON F. MCLENNAN has been appointed Professor of Psychology and Pedagogy at Oberlin College.

DR. T. L. BOLTON has been elected Instructor in Psychology in

the University of Nebraska to fill the place vacant by the death of Dr. Leon M. Solomons.

JUNE E. DOWNEY, A.M. (Chicago), has been appointed Instructor in Psychology at the University of Wyoming.

PROFESSOR J. H. TUFTS of the University of Chicago has been promoted from an Associate to a full Professorship of Philosophy.

DR. ALFRED VIERKANDT has qualified as Docent in Philosophy in the University of Berlin, with an inaugural address entitled 'Dualismus über psychophysischer Parallelismus.

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THE PSYCHOLOGICAL REVIEW.

ON RELATIONS OF TIME AND SPACE IN VISION.

BY PROFESSOR J. McKEEN CATTELL,
Columbia University.

In the ordinary vision of daily life the eyes, the head and the whole body are in continual movement. There are no distinct and lasting images on the retina; the physical conditions are those of the photographic plate when the camera is constantly moved hither and thither. But the world that we see appears to each of us distinct and unshifting. When I glance across the room—along a row of books covering its side, for example—images follow one another on each retinal element in rapid succession, but I see this time continuum as a space continuum with all the objects duly arranged side by side.

When black and white surfaces are moved across the retina with the color-wheel at the rate of fifty white stimuli per second, a gray, as every one knows, is seen. When, however, the eye moves so that the line of sight passes over black and white surfaces at the same rate, there is no fusion whatever, the surfaces being seen distinctly side by side. No fusion occurs even when 1,000 stimuli per second fall upon each retinal element.

When colored surfaces are moved one after the other across a limited part of the retina, under conditions described below, they do not seem to follow each other on the same field, but the colors are seen spread out side by side or intermingled in a larger field than is actually presented. The arrangement in space of the stimuli varies greatly with different observers, each seeing them in a way peculiar to himself. Thus in the elabora-

tion of a time series into a spatial continuum the same stimuli are followed by perceptions different for each observer.

These phenomena are important from several points of view. They indicate that the fusion of stimuli—and indeed all the phenomena of color vision—are not the result of chemical processes in the retina, but are cerebral phenomena dependent on complex factors. The individual differences under unusual experimental conditions indicate that the processes of vision are not organically fixed, but are the result of individual experience and adjustment. The perception of a time series as a spatial continuum shows that our perceptions are not 'copies' of a physical world, but are mental phenomena dependent on utility and the whole content of present and past experience.

Section I. Perception with the Moving Eye.

That in ordinary vision the spatial world of perception is reconstructed from a series of changes in time of the retinal images is an observation which, so far as I am aware, has not hitherto been made. But when once stated the fact is obvious and scarcely needs confirmation by special experiments. The results, however, when stated in quantitative terms, show a remarkable sensitiveness in the visual mechanism.

When the eyes are moved horizontally through an angle of about 45° once a second, from a distance of about 5 m., sweeping over an arc of about 4 m., on which are black and

Fig. 1. Black and white bars which do not fuse when the line of sight is moved over them.

white lines 2 mm. wide, as shown in the figure, stimulations at the rate of one thousand per second fall on each retinal area. There is no fusion into a gray, or blurring of the lines, so long

as the eyes are kept in focus for the arc. The white lines stimulating the retina in rapid succession are seen clearly side by side exactly as when viewed with the stationary eye, except that the field of distinct vision is enlarged.

When the eyes move so that the line of sight passes over the field at the rate of about 2 m. per second, going from a white to a black surface, it will in $\frac{1}{50}$ sec. move over 4 cm. According to our experience with the color-wheel there is fusion when one stimulation of the retina follows another at an interval of $\frac{1}{50}$ sec., or less, and if the same conditions obtained with the moving eye the white edge would not be seen, but white and gray would extend 4 cm. and more into the black. But as a matter of fact the white edge is seen with perfect distinctness, there being no fusion whatever. If one looks at the ground when riding in a trolley-car all the objects are fused, but it is easy to move the eyes so quickly in the opposite direction that the objects are distinctly seen. If when the car stops the line of sight is moved over the ground at the same rate there is no fusion whatever.

The rate of stimulation by movements of the eye cannot be increased greatly beyond that given above, and the limits of distinct vision are not set by the inability of the retina to respond to the stimuli, but by the 'minimum visible' and the maximum rate of movement.[1] More than a thousand interruptions per second give a series of sharply defined retinal processes. Vibrations fuse into a continuous tone when there are thirty or less per second, but two noises can be distinguished when separated by an interval of about .002 sec. Touches fuse at about the same interval. It is always stated that hearing and touch respond more accurately to successive changes in stimuli than vision, but this is apparently not the case.

The head can be moved more quickly than the eyes, and the whole body can also be turned more quickly than the eyes. I am not aware of attention having been called to this fact, which is of some interest in view of the phenomena here described. When

[1] Conflicting results as to the rate of movement of the eye have been obtained by Lamansky (*Pflüger's Archiv*, 2 : 418–422, 1869), Erdmann und Dodge (*Ueber das Lesen*, Halle, 1898) and Huey (*Am. Journ. of Psych.*, 2 : 283–302, 1900).

the head or body is turned quickly objects in the field of vision appear to move in a direction opposite to that of the line of sight, and this is the more marked the quicker the movement. When the eyes only are moved the field does not shift,[1] or if so the apparent motion is very slight. When the line of sight is moved by turning the head so that it passes over black and white bars at such a rate that there are about fifty stimuli per second there is no fusion. But when the movement is more rapid the field begins to move and there is then blurring and finally gray.

The fact that we see clearly and side by side objects over which the line of sight moves rapidly is evident. I have no doubt that the explanation I have given is correct—namely, that the visual mechanism is sufficiently sensitive to respond to more than 1,000 stimuli per second, and that we perceive a series of time changes on the retina as a spatial continuum because for our reactions it is in fact a spatial continuum. The only other possible explanations seem to be that the moving eye makes a series of jumps, seeing at each stop a distinct field, or that the distinctness of the field is an illusion, being really a memory image. But these suggestions are invalid. It can be readily observed that the eye does move continuously over the field. Further the field of distinct vision[2] is only about 1°, and to see all parts of an arc of 90° by separate steps the eye would need to stop some ninety times. It takes about $\frac{1}{5}$ sec. for the eye to stop and fix a field, and it is of course absurd to suppose that it could stop frequently in the half second needed to sweep over the arc. Finally if one had in fact only a few distinct images in the course of the movement it would remain true that a series of successive images is seen as a space continuum.[3]

[1] James (*Psychology*, 2: 173) is apparently mistaken in stating that it does, not discriminating movements of the eyes from movements of the head and trunk. The difference in the case of the moving eyes and the moving head may in part be due to the fact that with the eyes all objects in the field of vision maintain their relative positions, whereas when the head is moved nearer objects move over further ones, and we learn to regard a moving field as natural and a thing to be ignored.

[2] I do not find any measurements of the field of distinct vision, beyond the estimates of Weber and of Erdmann and Dodge. The fovea is said by Kölliker to be from 0.18 to 0.225 mm. in diameter, by Dimmer 1.1 mm. or more.

[3] In reading a line of printed text the eyes do stop as they sweep over the page. This, however, is not because there is any blurring of the letters with

The second alternative, that the distinct vision with the moving eye is an illusion, is at least conceivable. In daily life one does not notice that at each moment only a very small part of the field of vision is distinct. But this phenomenon is in fact explained by the constant movements of the eyes and the clear vision accompanying the movements—not conversely. Further it can be shown experimentally that when the eye sweeps over an object it is seen distinctly even though the observer may not know what it is. In this case if fusion occurred, as it does when the field is in motion, it would not, *e. g.*, be possible to discriminate red and green bars from white and black bars. Finally the perception as a space continuum of a series of changes in the time of stimulation is confirmed by the experiments with a moving field described below.

Section 2. The Fusion of Moving Objects.

The fact that the sense of vision responds to changes in stimulation as accurately as the other senses is not in itself surprising. I have always thought[1] that the phenomena of binocular vision, of contrast, of after-images, etc., are far too complicated to be attributed chiefly to the retina, and have never cared for the physiological mythology of the Young-Helmholtz and Hering theories of color vision. Assuming it to be true as a matter of experience that the moving eye responds accurately to changes in time of stimulation, it does not seem to me that this fact particularly requires explanation. On the contrary we need to explain why lights fuse when in motion. In this connection it may be suggested that vibrations producing a simple tone fuse at 30 or fewer per second, which is at nearly the same rate that black and white fuse on the color-wheel. When a slowly vibrating tuning-fork is touched with the finger we also have a partly continuous sensation. The conditions with these different senses are thus somewhat analogous. We have under certain

the moving eyes, but owing to the limited amount that can be simultaneously perceived and the time it takes to perceive it.

[1] I have said: "The writer finds most satisfaction in assuming that the continuity of physical vibration is transmitted through the retina to the brain, where inertia, summation and inhibition intervene to produce the changes which are correlated with consciousness." Psychol. Rev., 1, 326, 1894.

conditions fusion with comparatively few changes per second, and under other conditions can discriminate stimuli much closer together. I maintain that we should attribute the phenomena of perception chiefly to utility. We perceive what it is useful for us to perceive in order to direct our actions so as to preserve ourselves and attain our ends, and we usually perceive things in the way that is most conducive to these purposes. A general principle such as this does not of course obviate the need of investigating the conditions in the different cases, but it may lead us to consider that it is circumstances in which our senses fail in utility that need explanation rather than cases in which they fail to give copies of an external world, which is itself only the chief case of the interpretation of perceptions in accordance with maximum utility.

We can discriminate two noises separated by $\frac{1}{500}$ sec., but vibrations fuse into tones varying in quality when separated by much longer intervals. We notice very small differences in pitch, and the qualitative differences are evidently more useful to us than would be a series of sounds at varying rates. Beats on the other hand, though at the same rate as the vibrations giving tones, do not fuse perfectly.

In the case of vision a great loss in efficiency would result if the organs concerned could not respond to changes in the image as rapidly as these are brought about by movements of the eye. There would be confusion just at the time when clear vision might be of special importance. The processes of vision and of movement have developed in harmony, and the sensitiveness of the retina and of the brain centers is as great as is needed. The comparative slowness with which the eyes can be moved is perhaps due to the fact that a greater rapidity would be of no use, as it could not be followed by the retina and brain.

In the case of moving objects on the contrary, it is not necessary and would probably be disadvantageous for us to see the separate phases. The savage is concerned with the fact that a panther is springing toward him or a deer running away, not with the successive positions of the limbs of the creatures as instantaneous photography would exhibit them. Moving objects

in nature are seen as wholes as they should be for our convenience. When we produce motion by machinery or in the laboratory with the color-wheel we also naturally have fusion, but I doubt whether we do produce complete fusion with the color-wheel. It is assumed that we do, but I have never seen it. There is on the color-wheel always an appearance of glitter or translucence similar to that in binocular fusion. I have never been able to make gray in a color-wheel from red and green (with the necessary correction of blue), but when it is as nearly gray as it can be got I see both red and green with an appearance of translucence. Neither do I see a gray when red and green are mixed by reflection or diffraction; there is in this case an oscillation comparable to the conflict of the fields in binocular rivalry. If I am correct in these observations it is evident that we do not have complete chemical fusion on the retina.

I may refer in this connection to another phenomenon of the color-wheel, which I observed some years ago, but have not described. When we mix black with white we get a blue-gray. This is well known, and is said to be a case of the Purkinje phenomenon, according to which lights are said to become bluer as they are made fainter.[1] But if we throw a shadow on the same white that we are mixing with the black (say a perfect black from a black hole) we can get a gray evidently lighter or darker than the gray from fusion, but this cannot be matched, the shadow being yellow-gray as contrasted with the blue-gray on the color-wheel. The intermittent stimulation of the color-wheel seems to produce the blue,[2] and Talbot's law can scarcely be exactly correct. This experiment also indicates that in the fusion of successive stimuli we have not a chemical process in the retina, at least not such a simple one as is assumed.

The phenomena of after-images are not at variance, as might perhaps be supposed, with the ability of the eye to respond to very rapid time changes in the environment. A perceptible

[1] As a matter of fact the Purkinje phenomenon does not occur when the intensity of the colors is altered, but only when the eye is accommodated for a fainter light. I accidentally discovered this fact in trying to demonstrate the phenomenon to my classes in a light room. It has been described by Hering (*Pflüger's Archiv*, 60: 519–542, 1895).

[2] The degree of blue is, however, independent of the rate of intermission.

after-image is only produced by a certain minimum quantity of light,[1] and with the moving eye in ordinary vision the time that any object is fixed is extremely short, while the intensity of the light is usually small.[2] It is exactly the delicacy of the visual mechanism which would lead us to expect an after-image when the light is unusually intense or fixed for an unusually long time. It is a general principle that a nervous system sufficiently delicate to respond to ordinary stimuli is overstimulated and perhaps injured by extraordinary stimulation. Thus after-images, like the expression of the emotions in certain cases, are not useful reactions but pathological conditions. I can at present see an after-image obtained about six years ago.[3] This is not a purely retinal brand, because it can be made more distinct and, I think, permanent by acts of attention.[4] After-images do not in any way contradict the sensitiveness of the visual mechanism to rapid time changes, but on the contrary the indications that they are cerebral phenomena, and the fact that being useless they are ignored in our ordinary vision, support the general point of view of this paper.

Section 3. The Perception of Moving Objects.

When the color-wheel is turned so slowly that fusion does not occur there is a flicker if the sectors differ in intensity, and

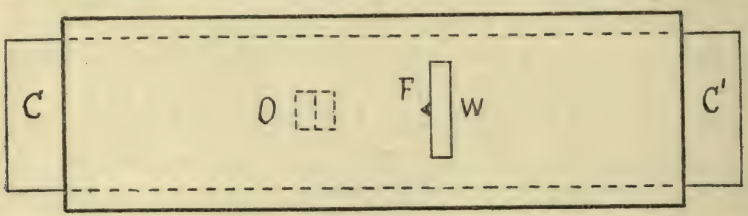


FIG. 2. Simple apparatus for studying the perception of moving objects.

[1] *Cf.* Franz, THE PSYCHOLOGICAL REVIEW, *Monograph Supplements*, No. 12, 1899.

[2] As a matter of fact the after-image appears only after a latent period, which contradicts the ordinary theory that it is simply the continuation of the commotion produced by the stimulus on the retina.

[3] *Cf.* my article on 'The Perception of Light' in *System of Diseases of the Eye*, Philadelphia, 1897.

[4] Newton made a similar observation. *Cf.* his letter to Locke in Brewster's *Memoirs of the Life of Sir I. Newton*, Edinburgh, 1855.

the colors intermingle. My research was originally concerned with experiments on moving objects and commingling colors, which led to the results on perception with the moving eye already described. Four methods were used to exhibit the stimuli, and it is desirable to describe these in some detail, as they may be used for various purposes.

(1) The phenomena may be produced by very simple means with sufficient accuracy for demonstration. All that is needed is a black screen about 30 cm. long and 10 cm. wide with a slit perpendicular to the long side 5 cm. long and

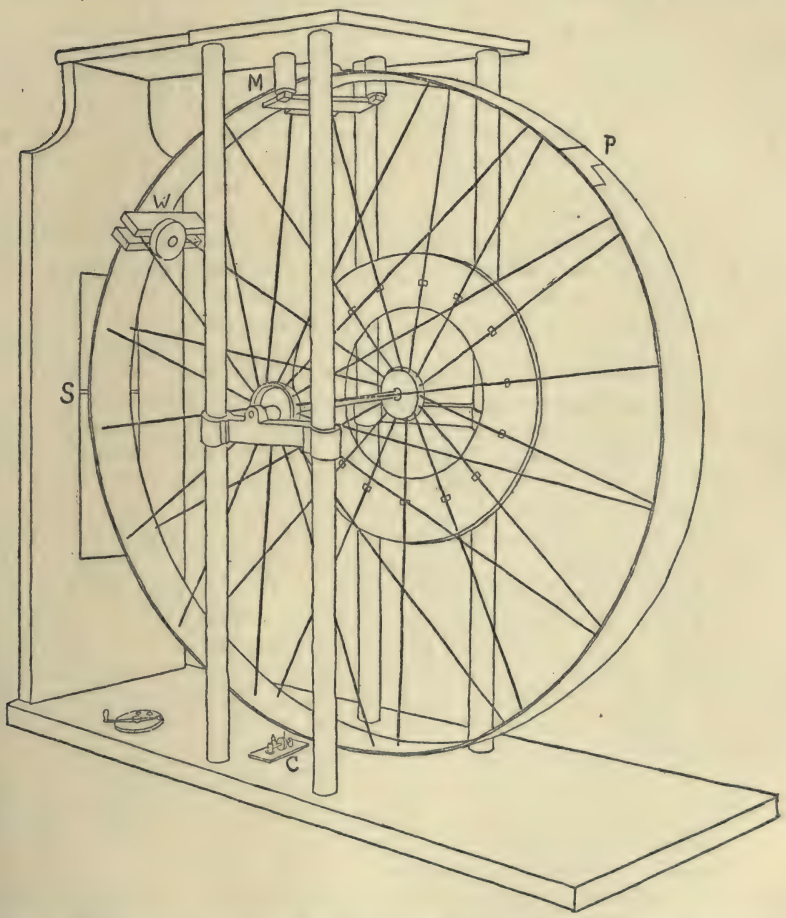


FIG. 3. Wheel chronoscope for studying the perception of moving objects. The rate of motion, size of field, etc., can be accurately adjusted.

1 cm. wide. The screen is so attached to a board that a long card (CC') can be slid beneath it. On the card are attached colors or shapes (O) which are seen as they pass the window (W). The card can be moved with the hand at rates varying up to about 5 m. per second with a mean variation of about one tenth.[1] Thus if green and red bars each one cm. wide be pasted on the card, green can be exhibited for about .01 sec. and the same retinal elements immediately afterwards stimulated by red for the same period.

(2) In order to secure greater accuracy and convenience in these experiments and for other psychological purposes I had constructed[2] an instrument which may be called a wheel chronoscope. It consists of a wheel, one meter in diameter with a rim about 9 cm. wide. Cards or colors placed on the outside of the rim pass by the slit (S) and may be seen by the observer on the far side of the instrument. It is thus possible to exhibit a color or a series of colors for an interval dependent on the rate at which the wheel moves and the size of the colored surfaces. The intensity of the light and the size of the field can also be varied, thus permitting experiments on the relations of time, area, intensity and color in vision. The instant at which the color reaches the slit can be recorded by keys at C, thus permitting experiments on the time of discrimination, etc. The wheel can be revolved at a constant rate by a motor or can be turned by the fall of the weight W. The latter method was used in the present experiments. The wheel is in this case held in position by the electro-magnet M. When the current is broken the weight falls and the wheel swings like a pendulum. At and near the dead point the rim is moving at a nearly uniform rate and at this point the colors pass the slit in the screen. The rate can be adjusted by the weight W, and was in these experiments 1 m. per second. The object can be readily attached to the rim at P out of sight of the observer and exhibited by breaking the current of the electro-magnet. On the return swing the wheel is caught up to the magnet, the slit being in the meanwhile covered. If a green surface 5 cm. wide is attached at F, followed by a red surface 5 cm. wide, the observer has over an area of the retina through the slit, say 1 cm. wide, first green exhibited for 50 σ ($\frac{1}{20}$ sec.) and then following it in the same area red for 50 σ.

(3) A chronograph[3] was also used to exhibit the colors. The horizontal drum with a circumference of 95 cm. could be rotated by a motor or by hand, at varying rates, usually so that this was about 1 m. per sec. The colors attached to the drum were exhibited as they passed a slit in a screen. An extension was also made to the drum so that the colors passed by a slit, and the colors only. In other cases with the wheel chronoscope or the chronograph, the black surface of the instrument was seen moving before the colors came into view. The black was as perfect as it could be made, but it suggested a moving surface, which has an influence on the perception.

(4) In order to do away with the moving background and to exhibit the phenomena simultaneously to a number of observers, the stereopticon was

[1] Fullerton and Cattell: *The Perception of Small Differences*, Philadelphia, 1892.

[2] By J. D. Brown, Camden, N. J. The instrument can be supplied by the instrument maker of the department of psychology of Columbia University.

[3] I was so fortunate as to buy three excellent chronographs for $10 each, they having been made for an unsuccessful system of synchronous telegraphy.

used. The colors were in this case gelatine films attached to a falling screen which dropped before the stereopticon and to a pendulum which swung before it. In this case there appeared on the screen first say green for 50 σ and then in the same area (the size of which could be regulated) red for 50 σ. This method is evidently useful, not only for demonstrating these phenomena, but also for exhibiting colors, words, etc., in various experiments to be made by all the students of a class. I have in this manner also demonstrated reaction-time, muscular fatigue, the effects of mental changes on the pulse and respiration, etc., some of the students making the experiments, and all seeing the results on the screen as they proceeded.

When moving objects are exhibited by the methods described, the resulting perceptions are in some respects analogous to those with the moving eye, and the experimental conditions make possible an analysis and more exact study of the phenomena.[1]

If an observer looks at a horizontal window one cm. wide in a black screen, and a black surface behind the screen moves at the rate of one meter per second, white paper and colors attached to the moving surface are seen as they pass by the window in the screen. If a piece of white paper is used 10 cm. in length it is in view for one-tenth of a second as it passes the window. If the eyes do not move, and they cannot make a movement in one-tenth of a second, a retinal field corresponding to the size of the window is stimulated by the light. The physical conditions are the front edge of the light moving over the field in one-hundredth of a second, then the stimulation of each retinal element for one-tenth of a second by the white light, and finally the second edge of the white moving over the field. We have a series of changes in time which might conceivably be followed by a corresponding series of time changes in the nervous system and in consciousness. As a matter of fact we know that the sensation does not exactly follow rapid changes in stimulation. Lights following one another at intervals of one-hundredth of a second fuse, and we should not expect to see the white surface moving over the field, but a fusion of the white and black. We also know that there is a time

[1] I may perhaps be permitted to make in this connection an incidental remark on behalf of experiment in psychology. The chief observations in this paper could have been made by any one at any time, but as a matter of fact they were not made except as the result of a quantitative experimental investigation, continued at intervals for about ten years.

threshold for vision.[1] If a white surface is exposed to view for a very short time it is not seen. When the time is, say, one-thousandth of a second, it is seen as a gray which increases in brightness until the time of exposure is one- to two-tenths of a second, when the sensation is a maximum. As the time of exposure is taken longer the sensation diminishes. Then we also know that after the objective light has been cut off the sensation may be followed by an after-image.

When consequently a white surface is exposed in the manner described, we might perhaps expect to see a field the size of the window, which would be at first gray and after gradually increasing in brightness would fade away. What I in fact see is a field larger than the window, very bright in the middle and shading through gray into black at each side. A process physically and physiologically in time is given in perception as a spatial continuum. This is evidently a phenomenon similar to that already described when with the moving eye objects successively stimulating the same retinal areas are seen side by side.

The experiment can be varied in many ways, and the results can be made more obvious. If the white surface is on one side 5 cm. and on the other 10 cm. in length we have the conditions shown below in Fig. 4. The left hand side of the field of vision is stimulated for one-twentieth, and the right hand side for one-tenth of a second, and the field is of the same size on both sides. The right hand side appears to me, however, to be perhaps 3 cm. broad, about twice as large as the left hand side and much brighter. There is a contrast effect, making the field darker near the middle on the left hand side and at the edges the white fades away indeterminately. In all these experiments a cloud-like or phantom-like image is seen which is difficult to draw or to describe.

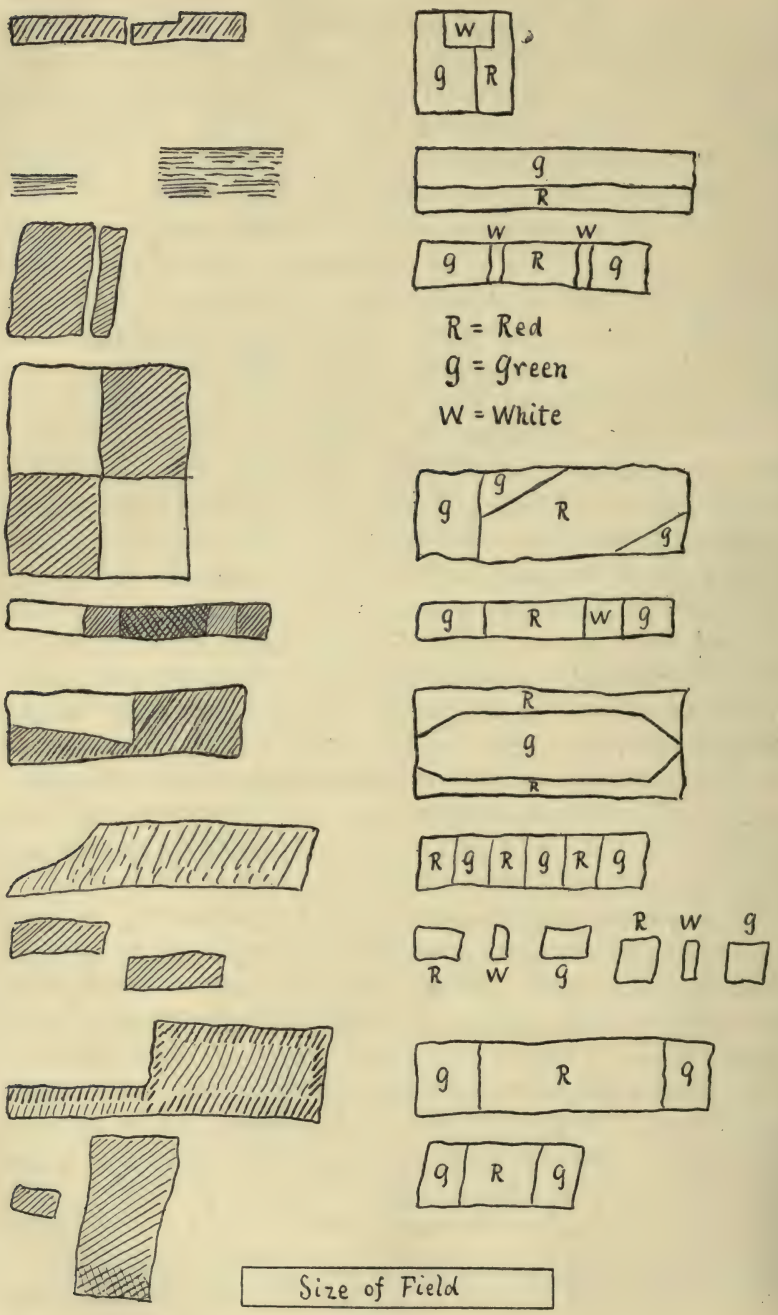
When colors are used—say green for one-twentieth of a second followed by red for the same time—we have the rapid moving across the field of the edges of the colors, and the retinal area stimulated first by red and then by green. The observer does not, however, see first green in the field, followed by a mixture,

[1] Cattell: 'Ueber die Trägheit der Netzhaut und des Sehcentrums,' *Philos. Stud.*, 3: 94–127, 1885.

followed by red, but the colors are seen simultaneously on a field larger than the window. I see below green fading away indeterminately into black with bright white in the middle commingling with green below and red above, the red fading into black. The white is brighter than white paper, much brighter than the gray obtained by mixing green and red on the color-wheel. The lines are never sharp, the colors being transfused, the one appearing spread over the other. The appearance of the image changes from time to time; after many experiments I do not now see the extension of the image beyond the size of the window as clearly as formerly, only seeing it distinctly when the moving areas are of different sizes on the two sides.

The experiments on moving objects exposed to view for a short time I have varied in many ways, regarding the rate of motion, the time of exposure, the area of exposure, the number of stimuli, the different colors in varying combinations, the shape of the field, the intensity of illumination, the motion of the field before the stimulus appears, movements of the eyes during exposure and other factors. With so many variables the details become complicated, and it is perhaps best to postpone their consideration, as they do not affect the chief conclusions of the present paper.

The individual variation of the perception is, however, a matter of fundamental importance. With exactly the same stimulus different observers have entirely different perceptions. When the moving field is exhibited for the first time to the observer under the conditions described, he not knowing the character of the stimulus, the perception is usually extremely vague, a mere cloud of light perhaps, the size, shade and brightness of which cannot be described. When the same stimulus is exhibited a second time it is usually perceived more distinctly. The attention has a more definite direction, and the memory image fuses with the effects of the stimulus. After three or four trials the observer ordinarily has a fairly definite perception, and then usually continues to have the same perception as the stimulus is repeated. There may, however, be a sudden change in the way in which the stimulus is perceived. When the observer is told what is actually exhibited or what others see, some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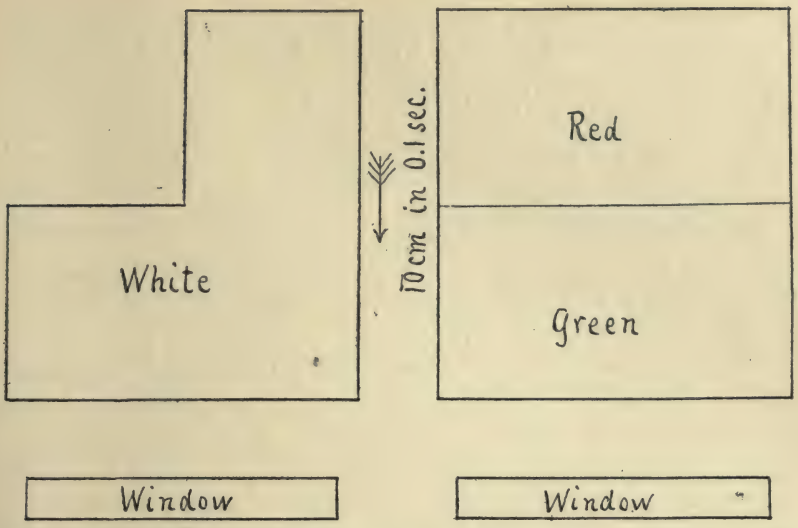

FIG. 4. The stimuli as described in the text are shown above. On the left hand page is shown the way these stimuli looked to ten different observers.

observers have the same perception as before, while with others it is completely changed.

In the accompanying figure the results of the fifth exposure with ten observers and two different stimuli are reproduced. The stimuli are shown on the right hand side of the figure—in the one case a white area 5 cm. broad on the one side and 10 cm. broad on the other, in the second case green 5 cm. broad followed by red 5 cm. broad. The rate of motion was 1 m. per sec. and the field was consequently exposed for 0.1 sec., the area of exposure was 1 cm. wide and 8 cm. long, viewed from a distance of about 30 cm. The drawings of the observers are reduced to one-half. They are more or less schematic, it being difficult or impossible for the observer to draw just what he sees, the outlines being less definite and sharp than the figures indicate.

I have not selected the most variant results, but those of ten advanced students tested consecutively. It is evident that the same stimulus after being exhibited five times is perceived by the observers in entirely different ways. As has already been stated, we have physically lights moving across the field of vision and lasting 0.1 sec. Physiologically we have a limited

field of the retina over which the light moves, and from each retinal element for 0.1 sec. a gradual increase in the commotion of the nervous system which gradually subsides. Nearly all the observers perceive a field larger than is exhibited and the time changes in the nervous system are given in consciousness as a space continuum, *e. g.*, the increase in the commotion of the nervous system as the time of stimulation is increased is perceived as a spatial shading from black through gray to white. But in the area of the figure and in the arrangement of the parts the observers differ completely.

With the stereopticon I have exhibited moving colors on the screen to classes of about thirty students of psychology and have secured results analogous to the above. It is more difficult to reproduce the apparent areas, as the screen is not at the same distance from the observer as the paper on which he draws or at the same distance from all the observers. But the general results are the same. Colors exhibited consecutively are seen simultaneously side by side, arranged and intermingled in very different ways.

These experiments show how absurd it would be to suppose that a perception is in any way a copy of the physical world or the correlate of a simple physiological process. The physical change is exactly the same and the physiological process is nearly the same for all observers, but the perceptions are entirely different. The artificial conditions of the experiment may perhaps give us some indication of what happens to each of us in learning to perceive the physical world as we do perceive it, and may also lead us to consider that there may be more individual variation in the perception of the same physical world than we commonly assume. The variations of different observers and of the same observer at different times indicate that the perception of a series of time changes in the retina as a spatial continuum must be learned by the individual, and may lead us to realize what a complete chaos the visual world must present to the infant.

In these experiments the eye cannot begin to move during the exposure of the stimulus, as the time is one-tenth of a second,

while it takes about two-tenths of a second for the eye to move in answer to a stimulus. If the time of exposure is as long as 0.2 sec. then the eye may move with the stimulus and the field of the retina stimulated may be larger than the window. Zöllner[1] observed that a circle moved under a narrow slit in a screen appears like an ellipse, and von Helmholtz[2] explains the fact by movements of the eye. He remarks: "Um die Figur erkennen zu können, muss man ihr eben mit dem Auge folgen." But this, as Zöllner himself had previously shown, is incorrect; the phenomenon may occur through movements of the eye or without any movements. If the circle is 5 cm. in diameter and the window 1 cm. in width, we see an ellipse wider than the slit or even a circle 5 cm. in diameter, although the eyes are perfectly stationary. The eyes can be kept stationary by fixing a white spot on the edge of the window; if the eyes move, the movement is betrayed by a motion of the after-image.

If three white squares (whose sides are 2 cm. and which are separated from each other by 2 cm.) are moved across the field of vision at the rate of about 10 cm. per second, and are viewed with the eyes motionless through a window one cm. wide, I see simultaneously the three squares crowded together and

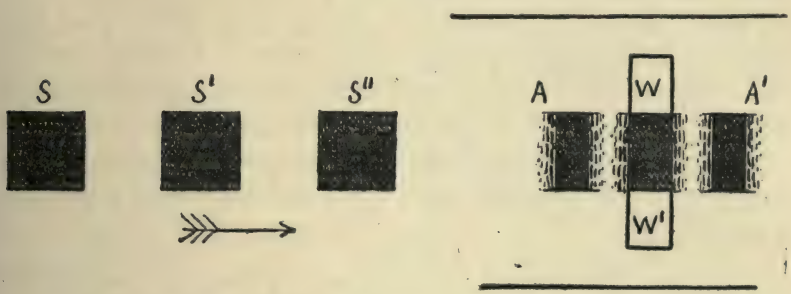

FIG. 5. The squares at the left appear as shown at the right when seen as they pass the window (*w w'*) of the size indicated.

blurred as shown in the figure. If the motion is more rapid the squares overlap, and finally coincide. If the motion is slower the

[1] *Poggendorff's Ann.*, 117 : 477–484, 1862. Since the present article was written a study of the illusion has been published by Mr. C. C. Stewart (*Am. Journ. of Psychol.*, 11 : 240-243, dated Jan., 1900).

[2] *Physiologische Optik*, 2d Ed., p. 749, 1895.

squares are seen to move across the field, but the perception is not that of one square replacing the preceding on the same field, but of three squares moving along and separated from each other by 2 cm.

These phenomena, again, are similar to those of perception with the moving eye. When the line of vision passes over objects which successively stimulate the same retinal areas we do not see a change in the same field, one object taking the place of the other, but the objects following each other in time are seen contiguous to each other in space. So when objects move and successively stimulate the same retinal area, they may be seen side by side and spread over a larger area than is stimulated. But there is the difference, already discussed, that with the moving eye there is no fusion or blurring, however rapid the change in stimulation, and the space relations of the objects remain the same whatever the rate of movement, whereas when the objects move there is partial fusion when the rate is 10 cm. per second, and the apparent space relations vary with the rate of motion.

The phenomena described for vision also hold for touch. If the finger is moved along a rough edge, as the teeth of a saw, the same points of the skin are touched consecutively, but an extended object is perceived. Here, as in the case of vision with the moving eye, we have the muscular sense as a factor in the total perception. But when the finger is stationary and the edge moves over the finger we have the perception not so much of one touch after another on the finger as of an extended moving object. In this case the illusion that the finger is moving is very strong.

Some of the results of this research may be summarized as follows:

(1) When objects are moved slowly over a limited visual field, while the eyes are stationary, they are not seen one after the other at the place where they are exhibited, but seem spread out over a larger area than can be seen. When successive white or colored surfaces pass over a limited field of vision so rapidly that the eye cannot be moved while they are in view, the stimuli do not seem to follow one another, but are perceived

simultaneously side by side, variously arranged and commingled. What is physically a stimulus lasting a certain time and physiologically a series of changes in time is psychologically an area in which the colors are spread out side by side, and the processes in time are a spatial continuum. The colors appear in this case to cover a larger area than is exhibited.

(2) Observers differ greatly in their perception of exactly the same physical stimulus under these artificial conditions. On the first exposure there is usually no definite perception, but as the experiment is repeated the observer tends to perceive the stimulus always in the same way, although two observers rarely or never have the same perception. The perception with some observers alters greatly when they know what the stimulus is or what others see; with other observers it is not altered. These phenomena indicate that visual perceptions are built up by the individual in the course of experience and may differ greatly in different individuals. For the infant the visual world is probably a chaos.

(3) When the eyes are moved so that the line of sight passes over objects, we have one after another stimulating the same retinal field, but we perceive the objects simultaneously side by side. A series of physiological processes in time is perceived as a spatial continuum. The same phenomena obtain for touch. In the case of vision the changes in the field may be very rapid—more than one thousand per second—and yet there is no fusion. We have, however, movement of the field and partial fusion when the head is shaken. The different results with the moving eye and with objects in motion (the retinal processes being the same) prove that the phenomena of fusion and color-vision are cerebral and mental rather than retinal.

(4) In general these experiments show that perceptions are not 'copies' of a physical world or the 'inner side' of special physiological processes, but are in large measure the result of experience and utility. The phenomena of vision as a whole require this conclusion, but the facts here described, showing that changes in time are perceived as extension in space, appear to be of fundamental importance for psychology and for epistemology.

JUDGMENTS OF MAGNITUDE BY COMPARISON WITH A MENTAL STANDARD.

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AND DR. EDWARD THORNDIKE,
Teachers College, Columbia University.

This paper presents the results of an experimental study of judgments of magnitude. The special aim was to discover the relation between the accuracy of these judgments and the amount judged when the basis of judgment was a mental standard. The judgments, that is, were such as one makes in ordinary life when he estimates the weight of a package as so many pounds, or the length of a table as so many inches.

The bulk of psychologists' investigations of the accuracy of our judgments of magnitudes has sought quantitative estimates of the amount of physical *difference* required in various cases to bring about a judgment of *difference*. Moreover the treatment of the results has been generally carried out with the intention of discovering *quanta* of sensation. The idea of an arithmetic of sensation units has often pervaded such studies to the exclusion of other real issues. It has been supposed that our judgments of difference in the intensity or the magnitude of two stimuli are the direct consequences of the presence in one case of an added quantum of sensation. If a subject choosing a weight as equal to a weight just lifted, made an average error of one-twentieth the amount, it has been supposed that he did so because the addition of a physical difference of less than that failed to give him any or a sufficient quantum of sensation. Fullerton and Cattell have shown the superficiality of this view.[1]

If we look at our judgments of difference we find that they may be based on a feeling of shock or tension felt in passing

[1] *The Perception of Small Differences*, 1892.

from one stimulus to another or on a similar shock of difference felt in passing from the associates called up by one to the associates called up by another. We may judge two lines to be unequal by the direct passage from one to the other, or we may have learned to call one twenty and the other twenty-one centimeters and may judge by the comparison of the two verbal associates. The *conditio sine qua non* of discrimination would seem to be the ability of the neural processes caused by the two stimuli to form connections with neural processes differing enough to give rise to a conscious judgment. If by associative connections the original processes can serve as excitants to more and more differing brain processes, it makes no odds whether *per se* they could give rise to a judgment of difference or not. And if such neural connections have been formed, the consciousness originally attending the first links may decrease or vanish without altering the success of the judgment. Attention may shift entirely from the bare sensation given. Feelings of its intensity or magnitude may play a very small part in the final judgment.

Let us now take the case of a judgment, not of difference or equality beween two stimuli, both readily accessible, but of magnitude in terms of some fixed unit not present to sense at the time. There is here no occasion for the feeling of direct sensory shock. Accordingly such judgments give us the means of determining the process of discrimination in a case where it is due to the associations. The mental process that occurs is this; one lifts a weight or looks at a line or an area or makes a pull or looks at an illuminated area and feels "It is 260 grams, or 140 mm., or 1 candle power," as the case may be. This feeling may seem satisfactory, or one may feel, in addition, "No! that is probably too much or too little; it is so and so." There is no conscious comparison with anything. Your judgment of the magnitude of the stimulus is like your judgment as to whether a certain object is a dog, horse or what not. The only difference is that you have a series of possibilities instead of a few alternatives. Of course, you may never reach a perfect association between the sense impression of the stimulus and the image of the words denoting its true magnitude, but may judge within a limit of error.

It seemed worth while to study the accuracy of such judgments because of their commonness in practical mental life, because of the possible value of getting at the facts on which Weber's law was based and by which it has since been disproved, from a new point of view, and because the naturalness of such judgments may make them better suited for individual tests than the artificial judgments necessary in tests by the methods of least noticeable difference and right and wrong cases. A number of minor points were brought out by the experiments which throw some light on the mental processes involved.

The method used (unless a special statement is made) was to guess at the length, or size, or weight of certain test weights, lines and areas, then to note the true magnitude and record the error. The weights, lines and areas used were intended to be indistinguishable in any other way than by their magnitudes. Precautions were taken which prevented the subject from knowing by the order of succession, or by the record he kept, which magnitude to expect or not to expect.

Judgments of Weight.

A series of test weights ranging from 200 to 500 grams by steps of 5 grams was used with subjects *H.* and *WH.* Each weight was, in the course of the experiment, lifted by *H.* ten

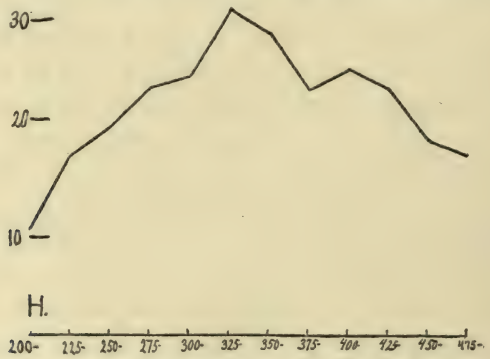


Fig. 1.

Judgments of weights. Subject, *H.* The ordinates represent the average error in grams. The figures along the abscissa represent the magnitude judged, *e. g.*, 200- equals 200 to 220 inclusive.

times, making 600 determinations in all. Before starting on the series the subject was given a practice period of 100 determinations, in order that the first few weights lifted in the test series might not be at a disadvantage.

The 310- and 335-gram weights were omitted in this experiment. Grouping each 50 judgments together and taking the average of the errors and plotting the curve we have the result shown in Fig. 1. Both constant and variable errors are included.

The shape of the curve is due partly to the fact that judgments at the extremes are freed from certain possibilities of error, but partly to unknown influences on the subject. He did not consciously judge by comparison with the extremes, as one might suppose.

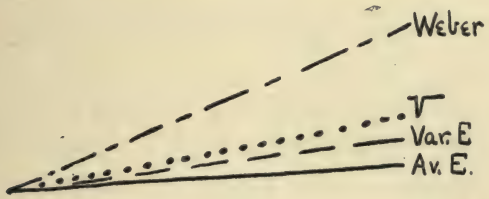


FIG. 2.

Judgments of weights. Subject, *H.* Comparison of the two halves of the series.

In Fig. 2 are shown (1) the average error and the variable error[1] for weights 200 to 350 and 350 to 500, (2) the error as it

[1] In general in these experiments the constant error is negligible or affects large and small magnitudes alike. No account is taken of it in this report except where its presence might cause an apparent inaccuracy, in large judg-

should be according to Weber's law and (3) the error as it should be according to the law of the combination of errors (the error increasing as the square root of the magnitude).

W. H., after a practice period of 50 determinations, judged the series of weights, making three judgments of each. Fig.

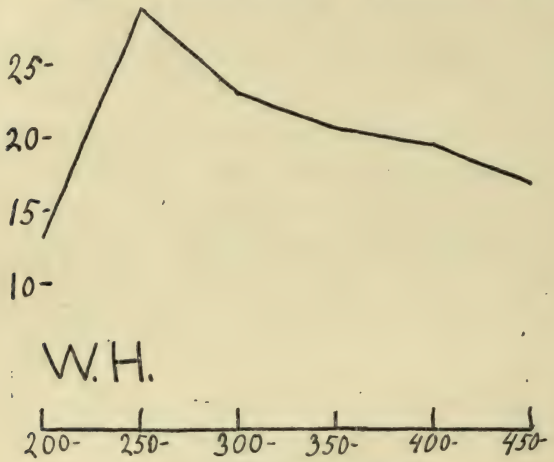


FIG. 3.

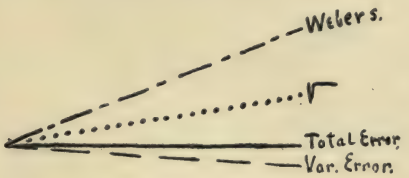
Judgments of weights. Subject, *WH.* The ordinates represent the average error in grams.

3 represents the average of his errors for the first ten weights, second ten, etc. Fig. 4 represents (1) his average error and variable error, for weights 200–350 and 350–500, (2) the error as it should be according to Weber's law and (3) the error as it should be according to the law of the combination of errors.

Subjects *W.* and *T.* were tested with a sense of weights ranging from 40 to 120 grams by steps of 5 grams. *W.* estimated each weight 50 times, and *T.* estimated each weight 100 times. Fig. 5 shows the relative inaccuracy of the judgments, the heights representing the average error, including the constant as well as variable error. No essential difference appears in a curve plotted from the variable errors alone. Obviously there

ments and not in small or vice versa. The tendency in *H.* to over-estimation of weights helped him with the heavier weights. We can discount this help roughly by grouping all his errors in judgments of weights from 200 to 350 grams, treating them as one series and finding the variable error, and then doing likewise with the errors of the judgments of weights from 355 to 500.

is here no correspondence whatever with Weber's law. The dotted line shows the error according to the law of the combination of errors.



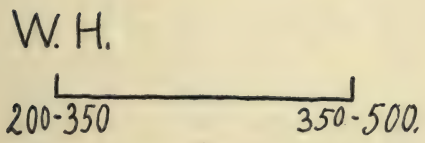


FIG. 4.

Judgments of weights. Subject, *WH.* Comparison of the two halves of the series.

Subject *T.* was tested later with a series running from 420 to 500 by steps of 5 grams, ten determinations being made with each weight. The average error of this series is compared with that of the first 10 determinations of the weights from 40 to 120

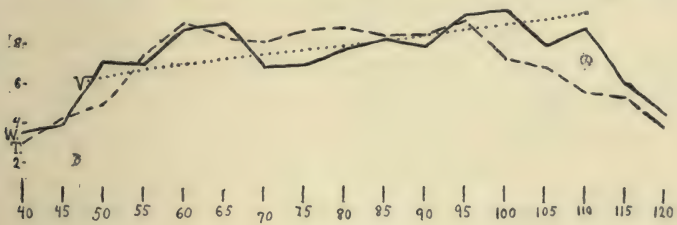


FIG. 5.

Judgments of weights. Subjects, *W.* and *T.* The ordinates represent the average error in grams.

in Fig. 6. Nothing definite can be said about the comparison, however, as the error is so large in comparison with the extent of the series.

These experiments with lifted weights are unanimous in showing no agreement whatever with Weber's law, and also in

showing if taken roughly a less rapid increase of inaccuracy than is in accordance with the law of the combination of errors. Probably in a new variety of experimentation like this some dis-

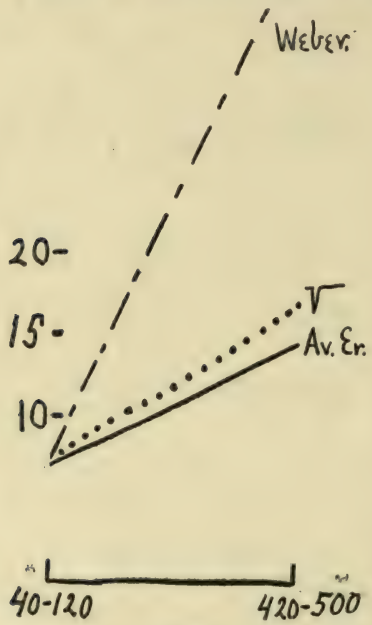


FIG. 6.

Judgments of weights. Comparison of series 40–120 with series 420–500. Subject, *T*.

turbing factors entered which the writers failed to note. However, we think these are slight.

JUDGMENTS OF LENGTH.

The apparatus for testing the accuracy of judgments of length consisted of a series of slips of paper of the same sort and equal width, of lengths of from 100 to 300 mm., varying by steps of 2 mm. There were two of each length. Subjects *H.* and *T.* were tested in the same manner as with weights. Fig. 7 presents the results of the experiments. The curve was plotted from the average errors of the estimates of lengths 100 to 118, 120 to 138, etc. *H.* had the knowledge of lengths due to having made the apparatus and the practice of ten es-

timates. *T.* had the practice of twenty estimates. *H.* judged directly from the gross length. *T.* consciously marked off 100 or 200 mm. with his eye and judged the remainder. As

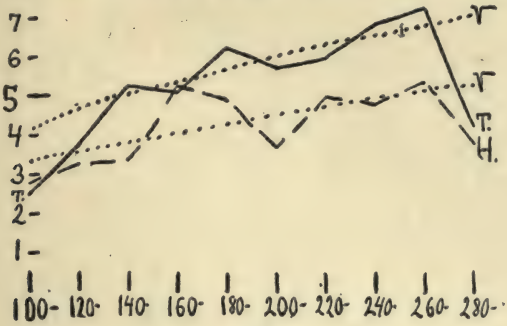


FIG. 7.

Judgments of length. Subjects, *H.* and *T.* The ordinates represent the average error in millimeters. The dotted lines are curves of the error as it would be if it followed the $\sqrt{}$ law. The continuous line is for *T.'s* error, the dash line for *H.'s*.

before, there is no approach to coincidence with the inaccuracy demanded by Weber's law.

JUDGMENTS OF AREA.

The judgments of area were made with the following apparatus: a series of parallelograms ranging from 10 to 140 and 190 to 280 sq. cm., varying each from the next by 1 sq. cm. Their proportions were almost the same (no one of them could possibly be distinguished by its shape). For example, the dimensions of those from 137 to 175 sq. cm. were

15 × 9.133⅓
15 × 9.2
15.1 × 9.2+
15.1 × 9.275
15.15 × 9.31
15.15 × 9.375
15.2 × 9.41
15.2 × 9.475
15.2 × 9.54

The error in area due to error in measuring and cutting them was tested roughly by superimposing, re-measuring and weighing them. It was always less than 0.1 sq. cm. in those

from 10 to 140 sq. cm. and always less than 0.2 sq. cm. in those from 190 to 280 sq. cm.

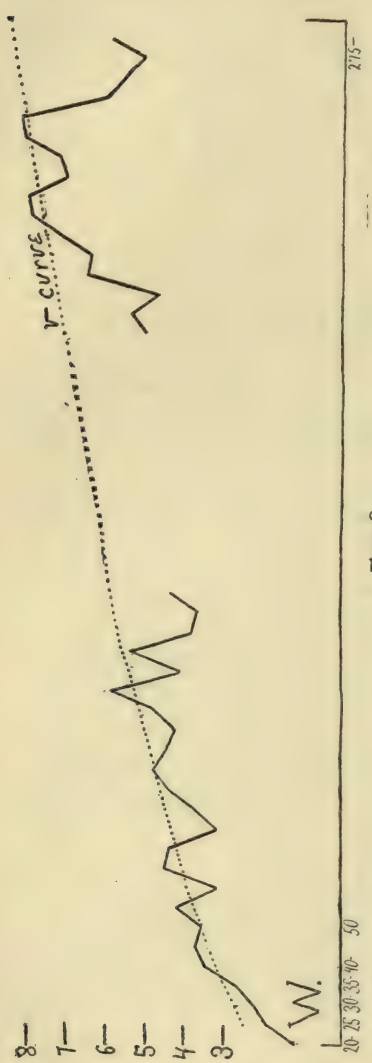


FIG. 8. Judgments of area. Subject, *W.* The ordinates represent his average errorin square centimeters.

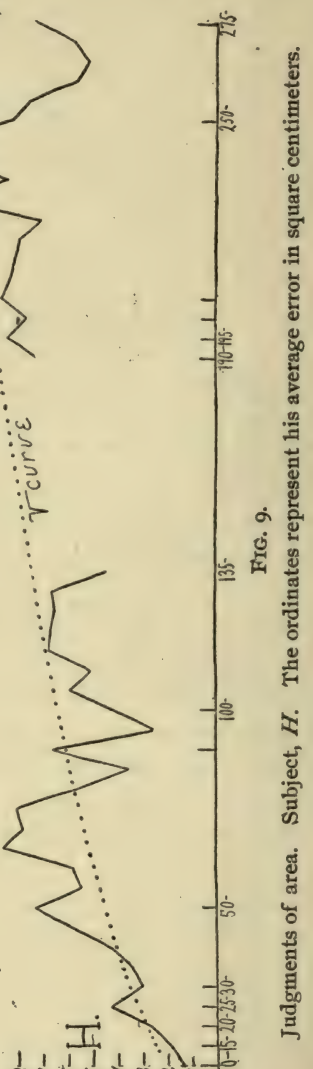


FIG. 9. Judgments of area. Subject, *H.* The ordinates represent his average error in square centimeters.

As in the other experiments, the subject picked out an area at random, judged its size, looked to see the real size and recorded his error.

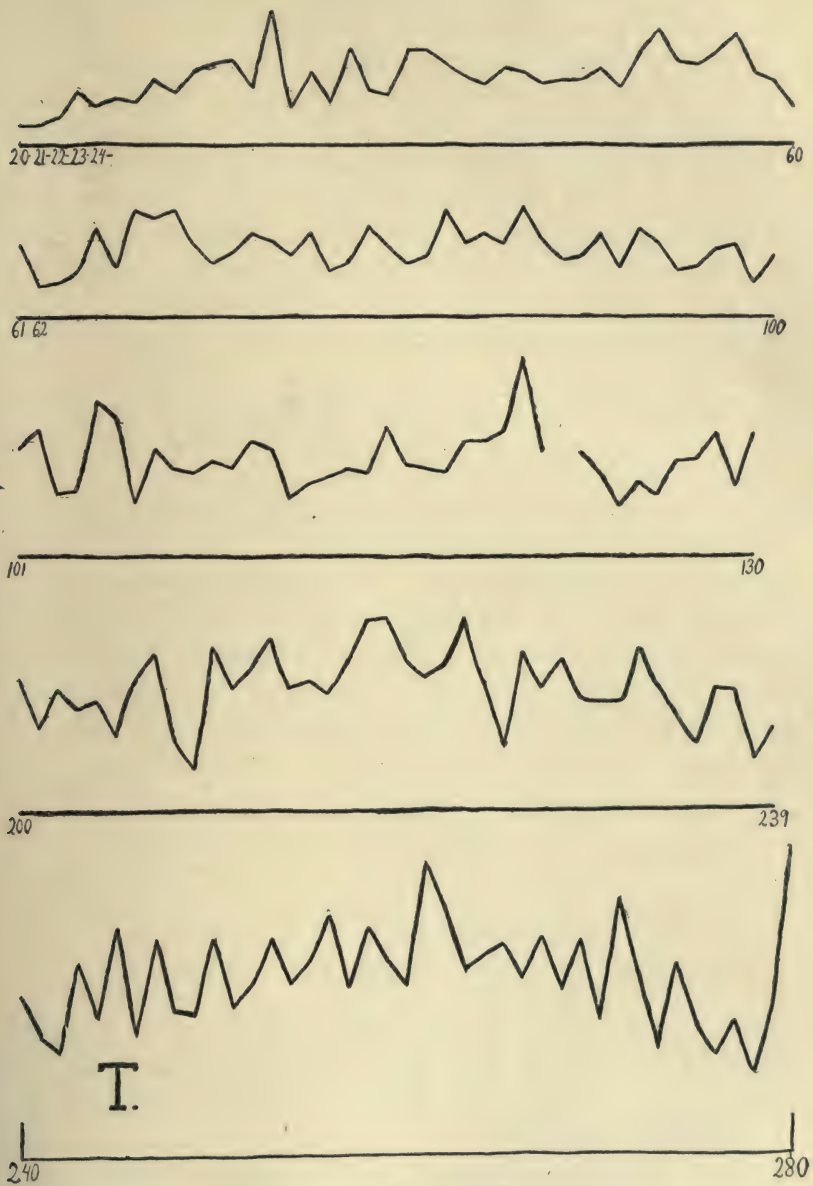


FIG. 10.

Judgments of area. Subject, *T*. The ordinates represent his average error in square centimeters.

After a practice period of fifty judgments *W.* made estimates of the areas from 20 to 140 and 200 to 280 sq. cm. In all, ten estimates of each were made. The work covered several days, but as elsewhere no one size was favored by practice, freshness or special effort.

Fig. 8 shows *W.'s* average error for areas 20–24, 25–29, 30–34, etc. The dotted line represents what the error should be in accordance with the law of the combination of errors. Leaving out of account the increased accuracy at the ends of the series, the actual errors follow the theoretical law fairly closely. There is evidently no agreement at all with Weber's law.

H. was tested with the full series 10–140 and 190–280 sq. cm. Five estimates of each were made. Fig. 9 shows his average error for areas 10–14, 15–19, 20–24, etc.

T. was tested with a number of smaller series one at a time. Ten estimates of each area were made. Fig. 10 shows his average error for each area. The series were the following and were given in this order: 20–60, 240–280, 61–100, 101–140, 200–239. These records are interesting in that they show no rise within any series, no marked rise save between 101–140 and 200–239. The slow increase from 60 to 140 in the

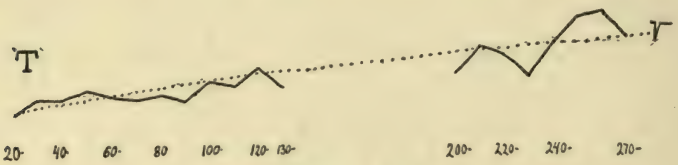


FIG. 11.

Judgments of area. Subject, *T.* A condensation of figure 10.

case of *H.* is paralleled here. The marked and extended increase in accuracy at the ends shown in the longer series is not found here. Fig. 11 shows *T.'s* errors when averaged for each ten successive areas.

It seems fair to conclude from these experiments as a whole (1) that the absence of any correspondence with Weber's law observed by Fullerton and Cattell in judgments of direct comparison holds true of judgments of comparison with a mental

standard. No subject in any of the experiments showed anything like any such increase of inaccuracy as Weber's law implies. Besides this, (2) the experiments show that in such judgments as we ordinarily make in life there are many factors besides the magnitude of the thing judged which affect the accuracy of the judgment, otherwise the form of the curve would not vary so much with different subjects.

A NEW EXPLANATION FOR THE ILLUSORY MOVEMENTS SEEN BY HELMHOLTZ ON THE ZÖLLNER DIAGRAM.

DR. A. H. PIERCE.

Amherst College.

Every one who has given any considerable attention to the Zöllner illusion is familiar with the strange gliding movements of the vertical columns which, under special circumstances, are

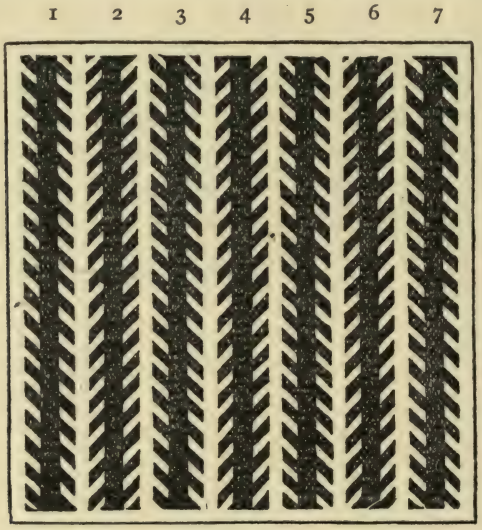


FIG. 1.

to be seen on the heavy-line pattern here figured. (Fig. 1.) Helmholtz was the first to describe these movements.[1] According to him, they are to be seen where one fixates the point of a needle moved horizontally across the diagram. If one be sufficiently practiced in following steadily a moving point a most striking and unusual phenomenon is presented. The vertical

[1] *Physiologische Optik* (2), 712.

columns are seen to shift their position with a graceful gliding movement in the direction of their length. Those verticals that bear upward-running transversals glide in one direction, while those with downward-running transversals pass as rapidly in the opposite direction. If the movement of the fixated point be from left to right, the former columns (2, 4 and 6 of Fig. 1) dart upwards, the latter downwards, just the reverse being the case when the moving point goes from right to left. Moving the fixated point alternately back and forth, and taking care to maintain an appropriately moderate rate of speed, there results a state of 'strange unrest' over the whole diagram, totally unlike anything else in the whole realm of geometrical optical illusions. One is reminded most forcibly, perhaps, of what might happen if the illusory creeping motion of the threads of rapidly rotating endless screws were presented to the eye in seven parallel lines and with alternate differences of direction. Not every one gets this illusion directly, but a brief practice in moving fixation, as well as some attention to the most favorable rate of movement and distance of diagram from the eye, will reveal it in all its vividness.

Now as to the existence and general nature of this illusion there can be no question. The apparent slippings and slidings of the columns are incontestable. It is only as to their *explanation* that there may be disagreement and discussion. It is accordingly the purpose of this paper to offer an entirely new explanation for these phenomena, an explanation which is based upon a consideration of the peculiar manner of stimulation experienced by the retina as the fixated eye passes over the diagram. That is, the explanation here given will be couched wholly in retinal terms, and not in terms of any elementary geometrical illusions which arise under the given conditions and which themselves cause the phenomena in question.

But let us see first what explanations are in the field. I have been able to find three: (*a*) Helmholtz's own is as follows:[1] The path of the moving eye is along a line which makes an acute angle with the oblique transversals which it crosses; or, more generally stated, every sensitive point of the

[1] *Loc. cit.*, 712.

retina is moving in a direction oblique to the actual obliques. There results a multitude of ideal acute angles, or "direction-differences," and since acute angles are always enlarged for perception, or what amounts to the same, since direction-differences are always magnified by contrast, the numerous transversals must appear to swing in such a way as to express this illusory angular enlargement. The upward-running transversals will consequently swing upwards, the downward-running downwards, and since any given system of transversals will move in concert, this illusory movement must be communicated to the vertical strips themselves. This is the substance of Helmholtz's explanation. It rests, plainly, upon a supposed angular illusion, the production of which draws the further illusion in its wake. Some of the minuter considerations, which in Helmholtz's view go to support this explanation, must be carefully examined later.

(*b*) Thiéry[1] claims to find the cause of these movements in the equivocal character of the prism surfaces upon which, to many observers, the vertical strips of the Zöllner pattern seem to be projected. To those gifted with the power to see perspective effects in linear drawings, these prism surfaces appear to tilt alternately backwards and forwards, and thus, on the perspective theory of optical illusions, the ordinary Zöllner illusion is produced. In the usual forms of the figure the intersections of these prism surfaces must of course be imagined lines lying in the *white* verticals between the black columns. Now these edges of intersection being imagined, as the eye moves horizontally over the figure, each successively fixated edge, Thiéry says, steps into the foreground—as fixated points or lines of equivocal figures always do—and thus each imagined intersection becomes in turn the front edge of a prism. By an immediate consequence the adjacent intersection on either side must retreat into the background, only to step forward once more when the moving fixation-point has reached the next intersection. It is this ceaseless movement backwards and forwards, consequent upon the movement of the eyes and due to the equivocal nature of the figure, that to Thiéry's mind is responsible for the 'strange unrest.'

[1] *Phil. Studien*, 1895, XI., 320-321.

Filehne[1] seems to entertain an identical opinion of the matter, though the expression of his opinion is nowhere explicit.

(*c*) Judd[2] adopts still another point of view by attempting to apply the observation of Müller-Lyer that if an acute and an obtuse angle have equal legs, those of the obtuse angle seem longer. Now, he says, the transversals of the Zöllner pattern make both acute and obtuse angles with the intersected verticals, and the horizontal movement of the eye, allowing, as it does, the successive fixation of the various points of the figure, permits the successive false estimations of the sides of these angles to come into prominence. Of course the mal-estimations that are effective for this illusion are those connected with the legs formed by the verticals themselves. And the particular slope of the transversals of a given column will determine the direction, up or down, in which this column will appear to move.

It will be seen that Helmholtz alone attributes any influence to the imaginary line drawn over the figure by the moving eye. The other two writers look upon the eye's movement merely as necessary to bring out characteristics latent in the figure, the conditions being thereby supplied for the arising of the further illusion of the gliding columns.

It is really remarkable that this curious illusion of motion has received so little serious attention. Either because this is less easy to see than the usual illusion that has engaged the almost exclusive attention of observers, or because the weight of Helmholtz's authority has tended to confine explanatory attempts within a particular realm of spatial phenomena, the true cause of this illusion, simple and near at hand as it is, has been persistently overlooked.

An Explanation in Terms of Peculiarities of Retinal Stimulation.

Suspicion against the current explanations may be readily aroused by noticing that the illusory movements are only faintly perceived upon the *light-line* models of the Zöllner figure. Attention is always directed to the original heavy-line diagram,

[1] *Zeitsch. f. Psychol.*, etc., 1898, XVII., 47–48.

[2] Psychological Review, 1899, VI., 260.

when this matter is under discussion. This would seem at once to indicate that purely retinal influences are powerfully operative in determining at least the vividness of the illusion.

My own suspicions were first awakened by accidentally noticing what happened when, with the diagram in full view, the convergence of the eyes was unthinkingly relaxed. As the eyes diverged the illusory movements began. They occurred again while the eyes were returning to converge upon the diagram. And if, when the optical axes were nearly parallel, there were slight movements to a state of greater or less divergence, the columns exhibited on their part their appropriate shiftings. In themselves, of course, these observations contributed nothing decisive to the problem in hand, but they served to arouse a line of investigation which has not been altogether without positive results.

It may be said here that for ease of observation a copy of the Zöllner pattern, like that of Fig. 1, should be pasted upon a small piece of cardboard of convenient size for handling. With this device one may demonstrate the Helmholtz phenomenon much more readily than by the usual method. Holding the column in any desired position—vertical, horizontal or at any angle—it is only necessary to shake the diagram slightly back and forth in its own plane and in a direction perpendicular to that of the columns to produce very vivid effects. The 'shaking' of the diagram is essentially the same as moving the eyes over it, for the eyes cannot readily follow its motions and consequently the image of the diagram moves over the retina, the result being the same for perception as if the retina moved over the diagram. The relations between objective and illusory movements in respect to their directions are the same with this as with the usual method of producing the illusion. The slippings and slidings of the columns to be seen under these circumstances strongly suggest that we are in the presence of phenomena analogous to those of the 'Fluttering Hearts.' There is a similar jelly-like movement here, but instead of a dim illumination the full daylight is requisite for the best effects. That the observation of the illusion is possible by the method just described would seem at least to show that Thiéry's explanation in

terms of the equivocal perception of prism-faces cannot find any reasonable application under the conditions here in force. No trace of such equivocal swingings of the prism surfaces is to be discovered.

Further, it becomes hard to reconcile the theory of Helmholtz with the observation that *the columns move in such a way that the obliques are everywhere parallel to their original direction*. That is, if the apparent movement of the columns is due to a principle of direction-contrast working upon the oblique transversals, or, in other words, if this movement is due to the illusory enlargement of acute angles successively formed by ideal lines passed over by the moving eye, there should be some vestige of a *rotary* motion observable on the obliques. For angular increase must take place about a vertex as center of rotation. And if such angular increase is the underlying cause of the phenomenon under consideration, it surely must be possible to perceive some *twist* in the transversals, some slight departure from their original direction. But as a matter of fact no such thing is to be seen. The columns move smoothly and evenly upwards and downwards, the obliques never changing in the slightest the course of their original slope. They rotate neither about the point of intersection with the verticals, nor about either of their ends. They move rather as if impelled by some push given to the verticals, to which they seem rigidly attached. If correct, this observation most certainly discredits Helmholtz's explanation.

And, finally, Judd's explanation in terms of mal-estimations of sides of angles is just as little able to maintain its claims. *For the verticals are not at all necessary for the illusory movements*. By cutting suitable strips from striped cloth or from properly ruled paper, the Zöllner pattern may be reproduced *without its verticals*. Under these circumstances the Helmholtz illusion persists unchanged in all particulars. But Judd's explanation is no longer applicable, for the sufficient reason that there are now no acute or obtuse angles whose sides are to be successively mal-estimated as the eye moves over the diagram. The main lines which supplied the indispensable condition for this explanation are no longer present, and it seems hardly

reasonable to suppose that imagined verticals may take the place of the actual lines that have been removed.[1]

It being now evident that no one of the current explanations can square with the simplest facts of the case, let us attempt the statement of the purely retinal theory promised above. Consider, for convenience, a single column of upward-running

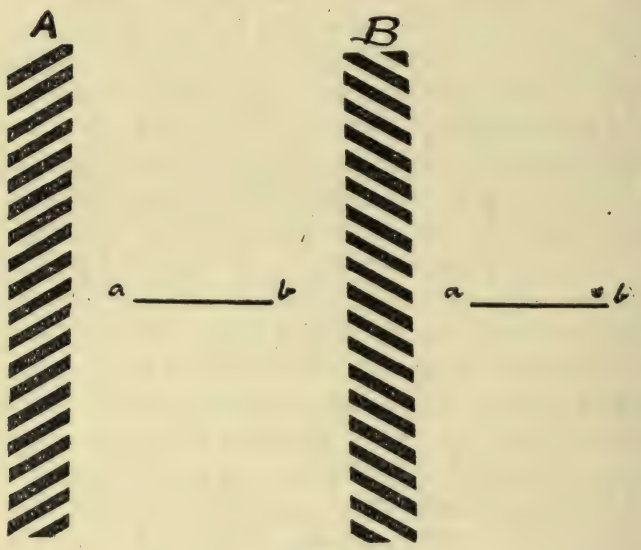


FIG. 2.

obliques without the central vertical (*A*, Fig. 2), and let the eyes be supposed to follow a point moving horizontally from left to right. That the illusion persists with only a *single* column may be easily ascertained by carrying out the usual movements of the eyes after all but one of the columns of the reg-

[1] The observation of Witasek (*Zeitsch. f. Psychol.*, etc., 1898, XIX., 153) to the effect that he observed no movement of the columns when the obliques alone were presented to one eye, though such movements were continually being observed on the complete diagram upon any slight movement of the eyes, was probably due among other things to the lightness of the lines he was observing. He was making a study of the usual Zöllner illusion under the conditions of stereoscopic combination—the verticals being presented to one eye and the obliques to the other—and under these circumstances the attention was presumably too fully occupied with other matters to notice any faint illusion. If, however, the eye had been moved intentionally over the part bearing the obliques only, the illusory movements could not have escaped his notice.

ular pattern have been covered. As to the position of the fixated point, experience shows that the clearest perceptions emerge when it moves along a line situated at the side of the column, along the line *ab*, that is, in Fig. 2. For my own eyes the most satisfactory results are obtained when this line is at the *right*. Others may find that the left side is preferable. In any case indirect vision seems best adapted to the perception of the illusion.

What happens under these circumstances, as the eyes follow the fixation-point along *ab*, may best be seen by considering the experience of a single vertical line of retinal elements. Let this line be thought of as resting on the extreme left edge of the column before us. It will then receive along its length alternate excitations of black and white. Now let the eyes move. As the supposed line of retinal elements passes from the left to the right edge of the column, the alternate excitations of black and white will fall higher and higher upon the line. The practical result for perception is therefore exactly as if numerous stimuli had *moved* up over a resting retina. This line of elements has experienced the full conditions for the perception of motion. But what happens to the single line happens in like manner to all vertical lines of elements that pass over the column of obliques, the upward-creeping excitations on each succeeding line being in each case lower down than those on the line ahead, as is of course determined by the slope of the stimulus-giving obliques. Manifestly the visual effect of this is entirely similar to the moving of the column bodily upwards over the resting eye.

If now the eyes move from right to left, the vertically arranged spots of stimuli, as we have pictured them, will travel downwards, and the visual appearance will be that of a descending column of obliques.

The directions of these vertical movements, it will be noticed, are in exact accord with the observations of Helmholtz. Columns bearing upward-running transversals run upwards when the eyes move to the right, downwards when the eyes move to the left.

Exactly the same style of considerations is to be applied to

the columns of form *B* (Fig. 2). With a movement of the eyes to the right successive stimulations will fall upon lower and lower points of the retina and the column as a whole will appear to move downwards, just the reverse being the case when the eyes move to the left. This again is wholly in accord with Helmholtz's observations.

The conclusion of the whole matter then is simply that the illusion under discussion is caused by the peculiar manner in which stimulations travel upon the retina. The horizontal movement of the eyes across lines lying oblique to their direction is equivalent in retinal terms to an ascending movement of the entire set of obliques over a resting retina. For perception the two processes have identical effects. Hence the illusion.

The general correctness of the view just expounded may be vividly brought out by the following procedure. Taking any piece of cloth or paper with closely lying stripes and placing upon this a piece of cardboard in such a way that the stripes run upwards across the vertical edge, move the cardboard in a direction perpendicular to its edge, the eyes meanwhile following some point on the latter. The effect will be that of an apparent ascending of the portions of the cloth successively uncovered by the moving cardboard. What occurs here is essentially the same as in the typical cases. Stimulations are mounting higher and higher along the vertical lines of the retina, and hence all the usual conditions for the perception of upward motion are fulfilled.

If the above explanation is correct, several important corollaries follow immmediately from it:

1. The *rate* of the illusory movement must depend upon the rate of the horizontal movement of the eyes. Helmholtz does not fail to notice this fact,[1] though he simply mentions it without discussion. It may be readily verified on the regular 'pattern' or upon isolated columns of obliques. Now on the theory of moving retinal stimulations this coincidence of rates *must* occur. The more rapid the horizontal movement of the eyes, the more rapidly will any bit of stimulus traverse its path rom end to end of any ob lique, and consequently the more illusory speed will the vertical column seem to possess.

[1] *Loc. cit.*, p. 713.

2. The *excursion* of the illusory movement must depend directly upon the *slope* of the oblique. This again is evidently a direct consequence of our explanation, for the vertical distance from tip to tip of each oblique increases with the slope of the latter, and accordingly the amount of vertical movement of the various stimulations along any retinal line, such as we have supposed above, must vary directly with the

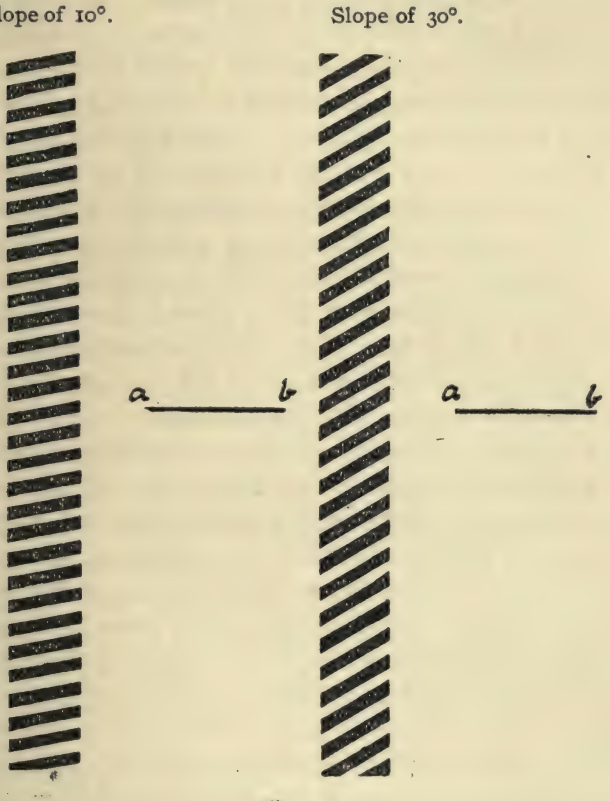


FIG. 3.

tilt of the oblique. To subject this deduction to the experimental test, cards were prepared bearing columns whose obliques sloped at angles of 10°, 20°, 30° and 40° respectively. At the side of each card, in the position indicated in the accompanying figure (*ab*, Fig. 3) a slit was cut which allowed the introduction from above of a common pin. The head of

the pin, moving upon the surface of the card, served as moving fixation-point for the eye. The length of each slit was 18 mm. Naturally the results obtained do not admit of quantitative statement, but *the anticipated increase of the illusion with the increase of the slope was everywhere realized.* The most striking way to bring this into evidence is to 'shake' the cards, in the manner described on page 360, the columns being held horizontally. There is no question that the columns whose obliques have a slope of 40° appear to traverse a much greater horizontal distance than those with 10° obliques.

Now it is difficult to see how Helmholtz's explanation can adequately account for this fact. Indeed it would seem that Helmholtz must expect exactly the reverse of what we have found. For in accordance with his principle that clearly recognizable differences are estimated as greater than those less clearly recognized,[1] it would seem that the angular enlargement due to direction-contrast would be greater for a slope of 10° than for one of 40°. For in the former case the difference of direction between the obliques and the imaginary paths traced by the eye must be clearer than in the latter case. Or, if not clearer, at least of a similar grade of clearness, which should cause the illusion to be equally great in the two cases. But it is unnecessary to attempt to strain Helmholtz's explanation into any form which shall pretend to cope with these facts in regard to the amount of the illusory displacement of the columns. These facts fall so readily into line with the retinal explanation proposed that they form one of its strongest confirmations, just as their absence would completely overthrow the theory here defended.

(*c*) Again, on the view here presented, the illusory movements should progress smoothly, without the slightest change of inclination on the part of the obliques. We have already emphasized the fact that this is so. And, in addition to the above, any slight movement of the eyes along the line of fixation should give a correspondingly slight illusion. And movements of the eyes by *stages*, that is with momentary stops between short movements, should be accompanied by an exactly parallel be-

[1] *Loc. cit.*, 705 and 714.

havior of the columns. Both of these deductions are unmistakably verified. Especially may one see what is meant here by substituting for the column of Fig. 3 a rather large patch of oblique lines. The movement of the eyes by tiny stages makes the patch appear to move stealthily up and down, creeping slightly along, then resting, then creeping further, etc. The whole impression during such an observation as this is that of some set of visual stimulations making their way gradually up and down the retina. Of the rotation of the obliques, which if it existed should assuredly become evident with these relatively long lines, not a trace is to be observed. There is the same smooth upward march that is to be seen in the case of the usual columns.

Another way of demonstrating this same thing is by diverging and converging the eyes upon the diagram as described above. This experiment may be varied by taking a single column from the 'pattern.' As the axes of the eyes diverge towards parallelism two columns are of course seen moving in opposite directions. But what interests us here is that every chance alteration in the degree of divergence produces a corresponding movement of the columns; slow, if the change in the eyes is slow, and of short excursion, if the change of divergence is slight. In short, when the eyes move there are immediate response and complete correspondence on the part of the moving column.

Now these facts can hardly be made harmonious with the thought of direction-contrasts that develop as the eyes move. Surely some length of movement must be requisite for the development of the consciousness of such direction-contrasts, and one should expect on this hypothesis, not the smooth movement that we have observed, but rather the progression of the columns by jerks, as the ideal horizontal line became long enough to evoke a feeling of difference in direction. Such jerky movements might not reveal themselves under the usually observed conditions of continuous eye-movement, but it would seem plausible at least to expect them under the special conditions here in force, were Helmholtz's explanation correct. The entire absence of such unsteady motion in the illusion, and the total impossibility

of discovering the faintest trace of rotation on the part of the obliques, combine to form a strong bit of evidence against the explanation here combated.

(*d*) That columns of upward-running obliques should run upwards, and that columns of downward-running obliques should run downwards, is an inevitable deduction from our premises.

Now Helmholtz records this fact with perfect clearness, but for some strange reason he does not attempt to show why either sort of column should have *its peculiar direction* of movement. And indeed on his theory this would be an awkward task. For instance, consider a single upward-running oblique. If the imaginary path, which is to establish the direction-contrast, be supposed to cut the oblique at its point of intersection with the vertical, the oblique must execute opposite movements with its two ends. With the eye passing to the right, the left end of the oblique must move down, the right end up. Which of these two movements is to be decisive for the final movement of the column? The one last seen? But why should not the movement first seen be just as effective, and consequently why should not the opposite sides of each column move in opposite directions? But there is a deeper difficulty. We have no basis for supposing that the imaginary path intersects the oblique at any particular point. All the sensitive points on one side of the retina are sweeping over the oblique and are cutting it at all conceivable places. Therefore, if for convenience we choose to mention a special point of intersection, we have no right to select one that is favorable to the result desired in preference to one that is absolutely unfavorable. For example, in our supposed case, we are in no way warranted in placing the point of intersection at the left end of the oblique rather than at the unfavorable right end. Under the former circumstances the angle would enlarge upwards, in the latter case downwards. In the one case we should have found an apparent explanation; in the other we should have met with a blank contradiction. The truth is that Helmholtz's theory is simply inapplicable. And may it not be that his silence in reference to the cause of particular directions of the illusory motions is not without important significance?

Though the evidence now adduced in favor of the purely retinal hypothesis may be regarded as sufficiently conclusive, one or two further points may give added weight.

The illusion does not always appear instantaneously when the eyes begin to move. A certain degree of retinal fatigue seems requisite for the full intensity of the phenomenon. This may be most effectively obtained by a moment's steady fixation of the point before beginning the movement of the eyes. This secures a slight after-image and provides a retinal point of reference for the ensuing movements of the various stimulations, and the illusion attains its most lively form.

At the same time a good illumination is a prime necessity. In a dim light the movement of the fixated point must be much slower than in a strong light, and the illusory movements are thereby greatly reduced in vivacity. Or if the movement of the eyes be the same for the two conditions of illumination, the illusion will almost completely vanish with a degree of illumination that is still sufficient for the clear perception and distinct discrimination of the lines when the eyes are at rest. Thus we find that the most advantageous conditions for the illusion itself are precisely those which are conducive to the most marked sensational effects.

A further point of interest is the heightening effect of *contrasting* illusory movements simultaneously present. In the Zöllner pattern these contrasts are already provided for, since alternate lines are moving in opposite directions. An isolated column, however, whether taken from the 'pattern' or constructed without the central vertical, gives an illusion of greatly diminished liveliness. Consequently in the experiments above described it is much more satisfactory to have a column of oppositely sloping obliques—most conveniently taken from the 'pattern'—ranged alongside of the particular column under investigation.

A peculiarity about these contrasting effects is that in the majority of cases only one column seems to move. This one column absorbs, as it were, and appropriates to itself whatever motion the other may have in the opposite direction. In my own case it is the nearer column which appears to stand still. This seems

to discharge its functions by acting as a reference column. As such, however, it undoubtedly contributes strikingly both to the clearness and to the apparent excursion of the illusory motion of its companion. This effect of contrast is prettily demonstrated by taking any pair of adjacent columns from the 'pattern' and then comparing the vividness of the illusion on that column which exhibits movement with the relatively feeble illusion resulting when the 'quiet' member of the pair is replaced by a column of short horizontal lines. The latter column might indeed be supposed to furnish a basis of reference, but its lack of illusory movement makes it practically without effect upon its companion.

It is, presumably, this principle of contrast which must be appealed to in explanation of certain minor phenomena to be observed when the complete diagram is 'shaken' before the eyes. Holding this in the horizontal position and shaking with a quick, short motion of the hand, all columns fall into their appropriate movements. But if the shaking movement be somewhat longer and less rapid, only columns 2, 4 and 6[1] will present the illusion. Now and then, possibly, the other columns will momentarily take on motion, but in general they seem to play the reinforcing rôle. That columns 2, 4 and 6 should display the illusion seems to depend upon the fact that they are most advantageously situated within the set of columns for receiving the full influence of contrast. That it does not depend upon the kind of the column, that is, upon the sort of slope that the obliques of 2, 4 and 6 happen to have, is shown by the fact that if a diagram be constructed by replacing each column by its opposite, the illusion will still be confined—at least most vividly—to the columns occupying the same positions as 2, 4 and 6.

It remains now to examine those widely accepted elementary phenomena upon which Helmholtz principally based his explanation. I refer to those oft-quoted cases, which the accompanying figure, Fig. 4, will call to mind immediately, where a direction-contrast, developing under the very eyes, graphically

[1] See Fig. 1.

performs its work of producing the overestimation of an acute angle.

Helmholtz's claim was, as every one knows, that, if the end of one leg of a pair of compasses be followed by the eye as it moves over the path *CD*, across the line *AB*, the two halves of the line will appear respectively to assume the positions indicated by the lines *aa'* and *bb'*. That is, the path of the com-

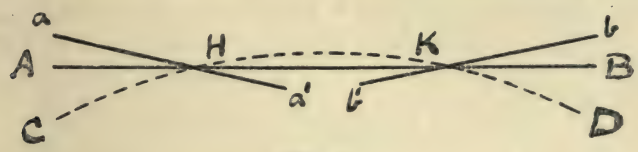


FIG. 4.

pass-point has made acute angles with the intersected line, these have been perceptually enlarged, and the left and right halves of *AB* have been seemingly forced out of position in the process of accommodating themselves to this unavoidable and insurmountable illusion of space perception. At first sight the phenomenon appears to be the isolated element of the illusory movements that we have been discussing above. The path of the compass-point as it cuts the line is the representative of the many paths swept out by the retinal elements as they intersect the obliques of the Zöllner pattern. And as either half of *AB* in Fig. 4 swings about the point of intersection, so the intersected obliques of the 'pattern' swing about and give the appearance of vertical motion to the columns. We have already seen plenty of reasons for rejecting this interpretation as applied to the larger phenomenon. And if now we are able to find some other interpretation for this supposed elementary illusion, all possible grounds for clinging to the explanation of Helmholtz will have been taken away. That an illusion of direction is to be seen when the conditions of Fig. 4 are fulfilled is unmistakable. But a really complete description of what happens seems never to have been given. For the more convenient study of the matter a heavy line of 4 mm. thickness and 50 mm. length was drawn upon paper and the latter fastened to the wall directly in front of the observer. A slender rod 475 mm. long and tipped with a steel knitting needle swung from a center be-

low in such a way that the tip of the needle traced an arc 22 mm. above the line at the highest point. This arrangement reproduces all the essentials of the original, while allowing greater facility in the way of control and observation.

What happens to the line AB as the point is carefully followed in its course is certainly not adequately represented by the usual oblique straight lines of Fig. 4. There is rather an apparent *sagging* of the line at the center, somewhat as represented in Fig. 5. And furthermore this whole appearance of

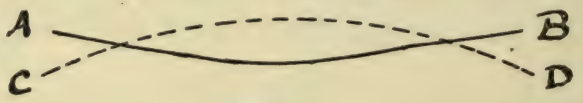


FIG. 5.

distortion may be produced by confining the moving point to the portion of the arc that lies between H and K (Fig. 4). That is, the actual intersection of the line by the path of the moving point is not at all essential to the illusion. This in itself is sufficient to cast an interesting suspicion on the hypothesis of direction-contrasts. For here we are obviously dealing with the path of the fixation-point, and not with ideal lines traced by any point whatever of the retina. A careful examination of the behavior of the line during the movement of the eye from H to K will, I think, reveal the following: If the moving point stop short of the crest of the curve, the line AB seems simply to move downwards, keeping always parallel to its original position. This is a necessary consequence of the eye's movement obliquely upwards, for the perceptual effect, or at least the retinal effect, is precisely what it would be were the line to move downwards and to the left. The sidewise component of the movement is not noticed unless the line has some conspicuous marks upon it. Again, if the moving point pass from a position just beyond the crest of the arc down across the line, the latter will seem simply to move upwards. In neither of these cases is any departure of the line from its original direction perceivable. But if now the moving point be followed throughout its entire course, or even if it be confined to the position HK, the sagging of the line appears. But this appearance is manifestly only a

necessary perceptual interpretation of three contradictory perceptions that come about successively as the point moves. First, while the eye is approaching the summit of the arc the line moves down, then as the summit is being passed it ceases altogether to move, and finally as the eye sweeps downwards the line moves upwards. That is, during the passage of the eye over the length of the curve, the line has presented three successive phases—upward movement, no movement, downward movement. Now bearing in mind that the attention is most sharply given to that portion of the line that is at any moment just below the fixation-point, and considering further that the final perception of the whole line must be made up of these three partial perceptions which follow each other so rapidly as to fuse into a single resultant, the illusory sagging line seems the only possible spatial form that could be perceived. The illusory sagging appears just because three incongruous perceptions are forced into one by the fact that the line AB must be perceived as a continuum, and the three modes of behavior of its three different parts must be apperceived as occurring upon an unbroken, continuous line.

There is no need of appealing to the influence of direction-contrasts here. We are merely in the presence of one of the so-called 'illusions of interpretation.' And as thus viewed this phenomenon has not the slightest connection with those other illusory movements which form the subject of this paper. Different causes are operative in the two cases, though both are reducible to relatively simple retinal happenings and need no underlying spatial peculiarities for their full and satisfactory explanation.

Helmholtz, it will be remembered, claimed to find evidence for the working of a direction-contrast when the path of a moving point is along a *straight* line—whether actual or ideal is indifferent—cutting a horizontal line at a small angle. The use of the compass-point arc in place of the straight-line path was, according to Helmholtz, simply to secure more vivid results. But I cannot refrain from thinking that Helmholtz's zeal to obtain a preconceived result led him here to fall into error. And I cannot find that others have verified the alleged observation that

the main line seems to incline away from the path of the moving fixation-point as the latter cuts across it. I have carefully examined the matter, using the same heavy line as above and letting the imaginary oblique be traced by the tip of a stiff wire fastened below to a tiny car, sliding upon an inclined plane. The moving tip was of course close to the plane of the paper bearing the line, and the angle of intersection was 5°. Now with an excursion of the moving point of about 10 cm. on each side of the line, nothing whatever beyond the usual and inevitable up-and-down movements of the line was to be seen. There was not the faintest vestige of tilting of the line. Nevertheless such tiltings were occasionally seen, yet so inconspicuous as to be easily overlooked, when the imaginary line was made as long as the horizontal itself. But this, once more, is only an illusion of perceptual interpretation. As said above in discussing the companion illusion, it is the portion of the line just above (or just below as the case may be) the moving fixation-point which seems most of all to be in movement. Accordingly when the eye, passing say from left to right, arrives at the end of its path, it is the extreme right end of the horizontal that is at that instant in most vivid apparent movement downwards. The rest of the line is still visible, of course, but it is relatively motionless. What other possible result then for perception than that the horizontal seems to be assuming a slight inclination downwards. The correctness of this view of the case is made overwhelmingly probable, it seems to me, by the fact that the illusory inclination of the line does not appear at about the time the line is being intersected, as we should expect on the theory of direction-contrast, but comes rather at the end of the whole path. Any one who will carefully repeat the experiment will be convinced, I feel sure, that the illusion here is due to the manner of interpreting motions on the retina and not to the over-estimation of angular magnitudes.

It may seem that these elementary phenomena have been dwelt upon at needless length. But I cannot help thinking that a real error has been firmly attached to them ever since Helmholtz first made his communications in respect to them. And since they have been so often referred to and so often made the

explanatory basis for other illusions which have no actual relationship with them, no amount of pains is too great which shall contribute to a more accurate understanding of their provoking cause.

Summary and Conclusion.

We have now the various facts and arguments before us. In conclusion these may be gathered together for convenient inspection. To begin with, we saw that no one of the current explanations for the movements to be seen on the Zöllner pattern could stand aright before certain easily verified facts connected with the illusion. That of Thiéry, in terms of equivocally perceivable prism-edges, could not meet the case where the illusion was produced by simply 'shaking' the diagram before the eyes. Grave discredit was seen to be cast upon Helmholtz's explanation by the observation that no change of inclination was to be seen on the part of the obliques during the presence of the illusion, though such change of inclination was supposed by Helmholtz to be the underlying cause of the apparent movements. And finally Judd's explanation was rendered inapplicable by the removal of the central verticals, there being thus no sides of angles to be falsely estimated.

The proposed explanation in terms of peculiarities of the movements of stimulations upon the retina was then supported by several considerations: First, that the rates of the actual and illusory movements correspond; second, that the excursion of the illusory movement depends directly upon the degree of the slope of the obliques; third, that the illusory movements, presenting, as they do, a behavior corresponding perfectly with that of the eye in respect to rest, movement and kind of movement, give the impression always of the passing of visual stimulations along the retina: and, lastly, that the particular directions in which the differently constructed columns move are now for the first time adequately accounted for.

And then finally those alleged basal phenomena in connection with single lines were seen to be entirely capable of a new interpretation, which removes them completely from any kinship with the more complex illusion which Helmholtz supposed them to explain.

Not only then have the curious movements of unrest that the Zöllner pattern may be made to show found an explanation that satisfactorily accounts for all the peculiarities connected with them, but in addition the fundamental basis for the original and most widely accepted explanation has been swept away. This new explanation, by being substituted for the old one, will probably have no far-reaching effect upon the interpretation of other geometrical optical illusions. Nevertheless it is decidedly worth while to get each individual illusion set in its true light.

ELEMENTS OF CONSCIOUS COMPLEXES.[1]

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Two conceptions, variously modified, lie at the basis of psychology. It may be treated after the fashion of Höffding and Brentano, of Stout, Royce and Baldwin, as a study of the activities or the relations of conscious selves; or else, as by Münsterberg, by Titchener and for the most part by Külpe, psychology may be regarded as a study of contents-of-consciousness, of percepts, images, feelings and the like, considered without explicit reference to the selves for which they exist. One of the most serious errors of the psychological theorist is the conviction that one of these methods must be 'right' to the exclusion of the other. On the contrary, both are valid and useful though they are entirely distinct. The psychology of selves in their relations is a genuine science, lying at the basis of history, ethics and philosophy, whereas the psychology of the contents-of-consciousness facilitates a close and helpful parallel of psychic facts with physical and physiological phenomena.

Equally misleading and far more frequent are the confusion of these methods and the alternations from one to another within the limits of one system. Wundt, for instance, adds to his analysis of *Vorstellungen* an uncoördinated doctrine of 'inner activity'; Külpe treats psychology, through four hundred pages, as a study of elements and their synthesis, and then suddenly swings over to the other point of view and considers consciousness-as-a-whole and attention as its state or condition (*Zustand*); and James oscillates without explanation between the two methods of regarding consciousness, now as a 'stream' of thoughts or a succession of 'feelings,' and again as a set of 'cognitive functions' or 'operations.' The ordinary division of the spoils between these methods of psychology allots to the study of

[1] Read, in part, before the American Psychologica Association, New Haven, 1899.

psychic contents the analytic treatment of percepts, of images and sometimes of emotions, but reserves for the other method the treatment of memory, of thought and of will. The truth is, however, that this confusion of two governing conceptions within one system is as unnecessary as it is misleading. Perception, as truly as will, may be treated as a form of self-consciousness; and, on the other hand, thoughts like images may be made to disclose their elements. In other words, every conscious experience may be studied from either point of view. The purpose of the present paper is, however, the consistent treatment of all psychological material from only one of these standpoints; the demonstration that every conscious experience may be treated as a content-of-consciousness and analyzed into its constituent elements. The immediate problem is the enumeration and the grouping of these elements.

The basis of the classification which follows is the rigorous conception of a psychic element as a distinct and absolutely irreducible part of a conscious content. This definition has been already outlined by the writer in a discussion of one of its corollaries; the impossibility that a sensation, if defined as element, should have attributes.[1] The present paper may therefore enter at once upon the consideration of certain objections to the theory which it upholds. The first of these sets forth that the term 'unanalyzable' has only relative value as applied to the content-of-consciousness. Experimental methods and careful introspection, it is stated, are constantly showing that experiences which seemed absolutely simple at the outset, are really further analyzable, as when, for instance, the supposed 'sensation' of humidity is found to be a mixture of tactual and temperature experience. The argument may be met, however, by showing that the admitted relativity and uncertainty, in the case of any given analysis, do not affect the logical accuracy of the conception of 'psychic element' nor alter its methodological value as a limiting concept. Indeed, every supposed psychic element might be shown to be decomposable, without disproving the conceivability of the element as such.

The second objection lays stress upon the artificiality and

[1] PSYCHOLOGICAL REVIEW, VI., 506, September, 1899.

the abstractness of the psychic element. It urges that the simple and undiscriminated content-of-consciousness is always first in actual experience, that the analysis of it is always a later, reflective process and that the fact-as-analyzed is radically different from that earlier undiscriminated content. All this, again, is freely granted. The element of consciousness is indeed only a distinguishable, not a concretely separable experience; the psychic fact is primarily unanalyzed; and analysis is a reflective, and not an immediate, procedure, never to be taken after the associationist fashion, as an account of the way in which contents of consciousness have been actually built up out of separate elements. Yet psychological analysis, through attention to the remembered parts of a psychic complex, though it is neither a primitive nor a universal experience, is nevertheless a necessary, scientific method and is employed by all psychologists to one degree or another. Consistency demands therefore that the analysis be carried out as far as possible; and the irreducible element is really no more radically unlike the concrete experience than the mixture of quality, intensity and extent which ordinarily goes by the name 'sensation.' Moreover, this complete analysis has practical value as well as theoretical necessity, for it stimulates close introspection and furnishes, as will be shown, a basis for the correlation of psychic with physical phenomena.

Quite opposed to this objection to the undue simplicity of our psychic element is the teaching of Professor Münsterberg's recent paper[1] that the 'really indivisible elements of mental facts' are not sensations nor in fact any observable phenomena of consciousness, but rather absolutely dissimilar physic atoms, which "no psychologist will ever observe in his own consciousness," since they are 'merely constructions for the purpose of explanation.' An adequate discussion of Münsterberg's argument would fall without the limits of this paper. Yet even if the psychic element—a seeming paradox—is an unobservable atom and therefore not an object of consciousness, it is nevertheless of advantage to consider what are the simplest phenomena of consciousness.

[1] PSYCHOLOGICAL REVIEW, VII., 10, January, 1900.

Assuming then that attentive introspection may discover elements of consciousness, we may next seek to formulate anew their distinguishing characteristics. The fundamental criterion of the element of consciousness is certainly this: its persistent distinctness from other elements and its unassailable simplicity. The element of consciousness is therefore the experience which regulated, repeated and verified introspection fails to analyze further or to identify with some other. This indeed is the only standard by which to estimate the element. The other criteria which we shall consider are clearly supplementary; they merely substantiate our introspective distinctions and assist us in classifying the elements which introspection has already disclosed. Our question, therefore, in its simplest form, is this: What sorts of elements do we actually distinguish?

I.

The group of psychic elements which we may first consider is the one admitted by all psychologists, the class of the sensational elements. 'Blue' and 'sour' and 'warm' and the rest, are unanalyzable yet distinguishable features of my experience and seem also entirely different from other elements, as for instance pleasantness. But besides this indispensable criterion of the elements—the observed distinctness and unanalyzableness—there are certain observable characteristics marking off the sensational elements from others. The first of these is the existence of a probable physical correlate for each sensational element. Thus, the rate of ether vibrations is the physical condition of each color; the amplitude of the vibrations is the condition of each brightness or color-intensity, the rate of atmosphere vibrations is the condition of each pitch, and so on with more or less exactness of nomenclature according to the degree of advance in the physical sciences. Sensational elements may further be distinguished from all others by the fact of their readily assignable physiological conditions and by the peripheral as well as central nature of these excitations. These marks of the sensational element are indeed so universally admitted that they do not demand detailed comment.

Reflective observation however discloses a final important

criterion. Sensational elements are present in every concrete experience, even when they are not predominant within it; and a given content-of-consciousness may conceivably consist entirely of sensations without the admixture of any other elements. A percept for instance need include no affective elements; it may be perfectly indifferent. It is this ability to stand alone, as it were, in consciousness which justifies the epithet 'substantive' as applied by James to sensations.

Within the class of sensational elements, thus marked off from others, psychological method distinguishes—under one name or another—qualities and intensities. (Extensities should be ranged under the head of qualities—if indeed they are not held to be complexes, including always motor or tactual experiences.) The question of the method of differentiating from each other these different sorts of sensational elements—the qualities and the intensities—is not altogether easy. There are of course the very clear physical and physiological distinctions. Extent of physical vibration and place of excitation condition the sensational quality, whereas amplitude of vibration and degree (with locality) of physiological excitation are the accompaniment of the sensational intensity. But another feature, a reflectively observed characteristic, distinguishes intensity from quality, by contrasting them in virtue of their possible serial arrangement.[1] Aside from what may be called the complex series in which sensational qualities may figure (color series like 'red, orange, yellow, yellow-green' or tone-series like C-CE-EG, in which the likeness of the successive terms is due to the presence of identical elements), sensational qualities are also capable of simple serial arrangement. Such series as 'red, yellow, green, blue,' or C-D-E-F are illustrations. Now the serial character of this succession is due to an increase not of the quality but of the difference. In other words the consciousness of 'more' which characterizes every step of a series attaches itself not directly to each quality but to the recognized likeness or difference of each quality as compared with its neighbors. Fully expressed, such a tone-series is not therefore C-D-E-F-G, nor yet:

[1] The theory of the series underlying the distinctions which follow will be recognized as that of William James. Cf. Principles, I., 489 *seq.*; 530 *seq.*

C
D = more C
E = more D
still more C

but rather,

C —
D — different from C
E — different from D
more different from C
F — different from E
more different from D
still more different from C

Intensities, on the other hand, are capable of direct serial arrangement. The increase is of the intensity, as it were, that is, the 'feeling of more' as James calls it, is directly connected with the consciousness of 'bright' or of 'loud' and our series becomes 'bright—more bright—still more bright,' or 'loud—more loud—still more loud,' and so on.

In all these ways, and fundamentally always by their immediately observed distinctness, the sensational elements are distinguished from the others, and within this group the qualities are contrasted with the intensities.

II.

The second group of elements of consciousness includes as its most undisputed subdivision the affections of pleasantness and unpleasantness, admitted by almost every one as elemental and distinct from sensations. Besides possessing the ultimate criterion of observed simplicity and distinctness, these are differentiated from sensations in that, in their case, there is no assignable physical stimulus—no definite form of physical energy which is the invariable accompaniment of pleasure or of unpleasantness; and because there is in all likelihood no distinct bodily organ by whose functioning each is conditioned. Theories of the physiological basis of the affections do indeed differ widely, but whether one hold with Titchener[1] that they are explained by bodily anabolism and catabolism, or with Marshall[2] that they are due to the pressure or absence of 'stored-up energy' in any one

[1] *Op. cit.*, § 31; Primer of Psychology, § 24.
[2] Pain, Pleasure and Æsthetics, p. 191 *seq.*; 222 *seq.*

organ, or with Münsterberg[1] that pleasantness and unpleasantness are 'values of a sensation parallel to the local situation of the discharge' of centrifugal or outgoing cortical energy—on any one of these theories it is evident that there is no definite 'organ' of pleasantness or of unpleasantness, in the sense in which the retina is the organ of color and the taste bulbs organs of taste; so that the physiological correlate of affections is a general condition independent of any particular organ.

In two important particulars of another sort, affections are distinguished from sensational elements. They are not always present and they are never conceivably alone in consciousness. One's perceptual or reflective experience may be utterly indifferent and devoid of affective coloring; and one can not imagine bare pleasantness or unpleasantness; there is always an agreeable or a disagreeable somewhat—a pleasant familiarity, or an unpleasant sound. Sensations, on the other hand, as we have seen, may conceivably occur without other elements in an indifferent and unrelated complexity. To mark these distinctions, affections may be called 'attributive' after the fashion in which sensations have been characterized as 'substantive.'

To the class of attributive elements, thus distinguished by the absence of physical stimulation, by the 'general' character of their physiological excitation and by their intermittent and coincident occurrence, other elements than the affections may be referred. The so-called 'feeling' of realness and the conation or 'feeling of effort,' if admitted at all as psychic elements, will be classed in this group. But the simplicity of each of these experiences is challenged by many observers. The alleged conation is treated as an anticipatory image, and the feeling of realness is held to be identical with the consciousness of congruity. Our purpose, however, is attained when we have assigned to these alleged contents of consciousness a purely hypothetical place among attributive elements, and we need not undertake to consider the mooted questions of their elementary character.

We shall omit discussion of still another problem, the possible distinction, within this class, between qualities and intensi-

[1] Psychology and Life, p. 94.

ties. Two theories are at least formally possible. The opposition between quality and intensity can, perhaps, be made only in the case of sensations; yet it is, perhaps, observable in every group of conscious elements. If this last be true, as certain experiences suggest, then pleasure-intensities must, of course, be distinguished from pleasure-qualities. Introspection, however, is at this point so difficult that the limits of our discussion preclude consideration of the question.

III.

With its next step, this classification departs definitely from the beaten track of psychological system. Yet the doctrine that the analysis of contents-of-consciousness is still incomplete, when substantive and attributive elements have been distinguished, lays claim to the unequivocal support of careful introspection. In the experience, for instance, of matching some one color with another, the blueness, the grayness and the brightness are not the only elements involved. On the contrary, the consciousness of the likeness or difference of the given blue to certain others is the very essence of the 'matching' consciousness.[1] Similarly, it is evident that a recognition is not exhaustively analyzed when one has enumerated all the sensations included in the immediate percept and the associated images, the organic sensations accompanying the bodily attitude and the pleasantness of the experience.[2] Neither any one of these, nor the combination of them, is what we mean by the 'feeling of familiarity' which marks the memory-image and the recognized percept. The experience of 'wholeness,' which characterizes our judgments, and the 'feeling' of 'anyness' or 'generality,' which on any conceptualist theory, is the essential feature of the general notion, are further illustrations of experiences so radically different from sensations and affections that they may neither be identified with them nor reduced to them. As James declares,[3] "we ought to say a feeling of

[1] Cf. H. Cornelius, Zeitschrift f. Psych. u. Phys., Bd. 22, 110.

[2] Cf. Titchener, Outline of Psychology, § 70 *seq.*

[3] Principles of Psychology, I., 245, end. It is almost unnecessary to remark that the word 'feeling' as used in this connection by Mr. James and by the writer, is a mere synonym for 'fact of consciousness' or 'psychosis,' and does not imply any admixture of affection.

and, a feeling of *if*, a feeling of *but* and a feeling of *by*, quite as readily as we say a feeling of *blue* or a feeling of *cold*."

These elements of consciousness may be described, for a reason which will later appear, as 'transitional."[1] The proof of their existence is, as has been indicated, the introspective discovery of actual experiences which simply are not analyzable into substantive and attributive elements. We do have, for instance, conscious contents corresponding with such words as 'different,' 'whole,' 'many,' 'more.' These certainly are 'affectively toned,' but the sense of pleasantness or unpleasantness which they subtly convey does not exhaust their meaning. The only sensational elements involved are those of the mere word-as-perceived; but the associationist contradicts the plainest results of introspection when he claims[2] that the verbal image is the distinguishing mark of such an experience. On the contrary, we realize that if the affection and the sensational verbal elements could be stripped away from such a conscious content—if, for instance, the verbal image 'like' and the characteristic pleasure of discovering similarities could be dropped out of the 'feeling of likeness'—its center and kernel would remain untouched, as a somewhat which, in the words of James, 'feels different' from any sensation or any affection.

A comparison of this class with the others shows that, as in the case of 'attributive' elements, there are here no physical stimuli, no forms of physical energy corresponding with experiences of 'oneness,' 'connection,' 'difference' and the like. But the probable physiological conditions are different from those of the affection, as of the sensation, for they are purely intracortical. Not a definite brain-center connected with a peripheral organ, not any general physiological condition of body-as-a-whole or of any organ, and not even an outgoing cortical impulse is the physiological explanation of the 'transitional' ele-

[1] James's epithet 'transitive' has not been employed, because its basis, the duration of the physiological process, has seemed unessential and not entirely certain. It has already been made evident that this classification departs at several points from that of James. He assigns no definite place to what we have called the 'attributive' elements, unless his 'substantive feelings' are meant to include them.

[2] Cf. Titchener, Outline, 85.

ment, which is conditioned, rather, by some spread of cortical energy, some excitation of transverse fibers within the brain.

There is still one other standpoint from which the 'transitional' element may be compared with the others. Unlike the sensation, but like the attributive element, it perhaps is not always present in a concrete content-of-consciousness. Far less frequent than the 'indifferent' experience, yet still conceivable, is the utterly undiscriminated experience, the conscious content without observed unity, multiplicity, likeness, difference—in a word, devoid of transitional elements. From attributive, as well as from substantive, elements most transitional elements are obviously distinguished by another characteristic: they require the presence of at least two other elements. One cannot be conscious of likeness without observing the two objects which are like, or of familiarity without being conscious of something present and of something past. The only apparent exception is the feeling of 'oneness,' and of this it may be said that it certainly must have originated in connection with a consciousness of 'plurality,' which evidently requires the presence of several elements.

The attempt to enumerate these 'transitional' elements discloses serious difficulties of introspection. The experiences in question have no physical conditions and are physiologically excited by central, without peripheral, change. It is therefore alike impossible to isolate a stimulus, or to bring an element into prominence by constant variation of its accompaniments. Contents-of-consciousness which are ideally analyzable may readily defy actual analysis and may be incorrectly treated as elemental. Examples of these delusively simple experiences are the feelings of 'wholeness' and of 'familiarity,' already named. Simple as they seem, they certainly are not strictly elemental. The first named may be reduced to a consciousness of one-as-connection-of-many and thus involves at least three perfectly distinct elements, and the 'feeling of familiarity' is even more complex, presupposing the consciousness of 'sameness' and of 'pastness,' each of which in turn is still further reducible.

Bearing in mind the admitted difficulty of introspective

analysis, the following enumeration of transitional elements is expressly offered as tentative and open to correction. The 'feelings' of 'one-ness' and of 'many-ness' or plurality are fundamental elements of this class, that is, they lie at the basis of most complexes. The 'feelings' of connection, of opposition, of likeness, of difference, of 'more-ness' and of 'less-ness' are probably also transitional elements, but this list may not be exhaustive and, on the other hand, it perhaps includes analyzable contents of consciousness. The 'feelings' of 'wholeness,' of 'necessary connection' and of 'sameness' are very simple combinations of these elements, readily mistaken, as we have seen, for unanalyzable experiences; and the 'feelings' of 'familiarity' and 'generality' or 'anyness' are still more complex contents-of-consciousness, but reducible to transitional elements.

This doctrine of transitional elements must of course contend against objections of two sorts. On the one hand, those who regard psychology as a study of functions of the self will cry out loudly against the peculiar sacrilege of reducing to mere conscious elements, to mind-stuff, as it were, the relating activities of Self or Mind. From the opposite camp, the upholders of the theory of psychology as mere study of psychic contents will come forth to do battle against a theory which, they say, admits Kantian categories, under the guise of 'transitional elements,' into the ranks of psychological facts.

To critics of the Intellectualist school the following reply must be made: The objection, if urged only at this point of our analysis, comes too late. If the right to analyze conscious contents into sensations, or into sensations and affections, has been admitted, as it practically always is, then consistency requires a continuation of the analysis until it is recognized as complete. It is unreasonable to say in effect: "While you are treating of perceptual and even of emotional experience, you may regard consciousness as a succession of ideas and of feelings and may analyze these into their elements, but when you come upon a judgment or a memory and are conscious of other than sensational and affective experience, you must change your point of view and drop your conception of elements and

talk only of the activities of Self or Mind." The method is valid throughout or it is absolutely invalid. Either it is incorrect, and not merely useless, to treat of percepts and emotions as combinations of sensational and affective elements, or it is also necessary, if introspection discloses unanalyzable contents like one-ness, connection and difference, to admit their elemental character.

The other difficulty is met by denying its assumption. Our transitional elements are not reflectively observed and classified 'categories,' they are experiences as immediate as the sensations or the affections themselves. The proof of their actual existence is the failure of all attempts to reduce memories and 'thoughts' to merely sensational and affective elements.

A very simple classification of conscious complexes may be based upon this distinction of psychic elements. Percepts and images, complexes in which sensational elements predominate, may be contrasted with feelings and emotions, complexes in which affections are the essential feature; and both may be distinguished from those conscious complexes which are characterized by the presence of transitional elements, or by simple combinations of them. These are: the judgment, whose essential feature is the 'feeling' of wholeness; the general notion characterized by the 'feeling' of generality or 'anyness'; and the memory image, in which the 'feeling' of familiarity is of paramount importance. All this is suggested by the following tabulation which summarizes the most important conclusions of the discussion:

A. Elements of Consciousness.

Criterion: Distinctness and Unanalyzableness.

I. 'Substantive' or Sensational Elements.

Criteria
1. (Psychological) Conceivable presence of elements of another order.
2. (Physiological) Definite peripheral and central excitation.
3. (Physical) Definite form of physical stimulation.

a. Sensational 'qualities.'

Criteria
1. Not directly accompanied by 'feeling' of 'more' or 'less.'
2. Varying with locality of physiological excitation.
3. Varying with rate of physical vibration.

b. Sensational 'intensities.'

Criteria
1. Directly accompanied by 'feeling' of 'more,' etc.
2. Varying with *degree* (and with locality) of physiological excitation.
3. Varying with amplitude of physical vibration.

II. 'Attributive' elements.

Criteria
1. Not always present and not conceivably occurring alone.
2. Physiological excitation: 'General' bodily condition.
3. No characteristic physical stimulaton.

a. Affections.

? b. 'Feeling of realness.'

?? c. Conation.

III. 'Transitional' elements.

Criteria
1. (*a*) Not always present and not conceivably occurring alone.
   (*b*) Requiring, each, the presence of at least two other elements.
2. Physiological excitation: intra-cortical.
3. No characteristic physical stimulation.

B. Complex Contents of Consciousness.

I. 'Substantive' (Sensational) experience predominates: { Percept / Image }

II. 'Attributive' experience predominates.

a. Affections: Emotions, etc.

b. 'Feeling of realness': { The Belief / The Volition }

etc.

III. 'Transitional' experience predominates.

a. 'Feeling of wholeness': The Judgment

b. 'Feeling of familiarity': { The Recognized / The Remembered }

c. 'Feeling of generality': The General Notion

DISCUSSION AND REPORTS.

THE FUNCTIONAL SIGNIFICANCE OF THE TERMS 'SENSORY' AND 'MOTOR.'

In another place[1] the writer has called attention to the existing ambiguity in the use of the terms 'sensory' and 'motor' in psychology. It was there pointed out, in connection with the study of sensory and motor errors, in reading and listening on the one hand and in speaking and writing on the other, that is, in connection with errors in the interpretation of the meaning of external symbols and in the expression of meaning in external symbols, that in all so-called sensory process there are motor, and in all so-called motor process there are sensory factors. Much confusion in the study of the psychology of speech, it was there remarked, has arisen from a false antithesis of the 'sensory' and 'motor' processes. And it was there maintained that the terms 'sensory' and 'motor' in psychology, properly used, are not content, but functional distinctions. It is the aim of the present article to aid in an understanding of the true functional significance of these terms. The subject has already been treated from somewhat different standpoints by two other writers, J. J. Biervliet[2] and J. R. Angell.[3] References will be made below to their discussions of the subject.

The words 'sensory' and 'motor' were first used in biology, in a purely descriptive sense. Charles Bell used the terms to characterize the afferent and efferent nerve bundles. This use became current in anatomical description, and found its way ultimately into existing psychological treatises through the channel of physiological psychology, which insisted on descriptions of the nervous system as preliminary to the treatment of psychology proper. Along with the

[1] A Study of Lapses, Monogr. Supplem. to the PSY. REV., Vol. III., No. 4, pp. 16 f., and 56 f.

[2] Images Sensitives et Images Motrices, *Revue Philosophique*, Vol. XLIV., pp. 113–128.

[3] J. R. Angell and A. W. Moore, Reaction-Time ; A Study in Attention and Habit, PSY. REV., Vol. III., No. 3, May, 1896, p. 254 f. Also, J. R. Angell, in a Review of S. N. Patten's 'Development of English Thought,' *Am. Jour. of Sociol.*, Vol. III., No. 6, pp. 824 f.

genetic and functional movement in psychology greater attention has been paid to the physiological, and especially to the neurological corollaries of psychical process. As a natural consequence, the terminology of biology has invaded psychology, for the most part with beneficial results, but sometimes producing a certain amount of confusion as the result of the mixing of two alien nomenclatures. It is beyond dispute that the application to psychology of the methods of natural science has been most fruitful, but the new terminology which has accompanied this change has not been assimilated, in some instances, and then stands as an unnecessary barrier to a more intimate interaction of the two sciences. The terms 'sensory' and 'motor' are a case in point.

It is important to have a clear statement of the principle underlying the proper use of these terms because on this distinction hinges the entire significance of certain recent or pending controversies. Among these may be named the controversy over the so-called 'innervation' sensations, the controversy between the intellectualists and voluntarists, the controversy between the representatives of the bipartite and tripartite theories of the fundamental modes of consciousness, the controversy between those who call themselves the structuralists and functionalists in psychology, and the various controversies which have taken place and are taking place over the interpretation of the facts of aphasia. It is not our purpose in this place to show how this ambiguity runs through all these controversies. We shall attempt rather to show where the fundamental difficulty lies, leaving this detailed application for another time and place.

There is no quarrel with the purely descriptive use of the terms. It is when they come to be given an explanatory or interpretative significance that the possibility of error comes in. On this side, we select two typical illustrations of the ambiguity. These are found (1) in the current theories of aphasia, and (2) in the customary characterization of the kinæsthetic sensations as 'motor.' We choose these two uses of the terms for criticism because here the ambiguity stands out most obviously.

The ambiguity of calling certain forms of aphasia 'sensory' and others 'motor' becomes apparent if one attempts to gain a coherent notion of aphasia from the current literature on the subject. This ambiguity grows partly out of false theories of cerebral localization and partly out of an imperfect psychological analysis. Flechsig has shown us that the centers in the so-called sensory region of the cortex are no more sensory than motor, that, in fact, they are sensori-

motor. An understanding of this point clears up the ambiguity arising from the anatomical side. On the other hand, a more mature psychological analysis of the facts of aphasia shows that the imagery involved in the so-called 'motor' types of aphasia is as much sensory as the imagery involved in the so-called 'sensory' types of aphasia. Aphasia is either all motor or it is all sensory; it cannot, from the same point of view, correctly be described as part sensory and part motor.[1]

The other illustration of the ambiguous use of the terms 'sensory' and 'motor' is found in the customary characterization of the kinæsthetic imagery as 'motor.'[2] These are the sensations which are connected with the movement of muscles, tendons and joints. The ambiguity lies in the tendency of various writers to regard the kinæsthetic sensations as motor in some sense in which other sensations are not. This is done, apparently, in an uncritical way and for no other reason than that the kinæsthetic sensations are connected with the motor organs. That imagery is called motor which is connected with the use of these organs of expression, that is, which comes from the sense organs located in the muscles, tendons and joints. The additional idea is frequently connected with this, that the kinæsthetic imagery is in a peculiar sense motor because it most immediately serves as motor cue, that is, it is the kinæsthetic imagery which immediately precedes the centrifugal discharge whieh sets free the overt movement. But on the one hand, as we shall see, the fact that certain sensations are connected with the activity of organs of expression (muscles, etc.) entitles them in no way to the peculiar characterization of 'motor,' and, on the other hand, if the kinæsthetic imagery is motor because it is essential to the motor response, then all the other imagery must be motor as well, since all sensations and imagery have this same dynamogenic tendency.

In order to unify the critical remarks which have just been made and to show upon what they are based, it may be well to summarize what we conceive to be the functional unit of interpretation in both physiology and psychology. We may take as the point of departure for our constructive statement Professor Dewey's critique of the concept of the reflex arc.[3] His substitute for the old idea of the reflex

[1] Collins in his 'Faculty of Speech' (1898) recognizes the ambiguity here, but he has not the courage of his convictions. Cf. pp. 4, 55, 57, 60, 160 f.

[2] Cf., for example, G. F. Stout, Manual of Psychology, 1899, p. 190 f, and 464 f.

[3] PSYCHOLOGICAL REVIEW, July, 1896, III., No. 4, pp. 357–370.

arc is the concept of the organic circuit. By this is meant that entire process of coördination, adaptation, adjustment, to use the physiological terms, or mediation, reconstruction, reorganization, to use the psychological terms, which includes within it as functions or part-processes both the stimulation and the response, the excitation and reaction. The process of stimulation includes all the stimuli which factor in securing a response; both internal and external, organic and extra-organic. The truth is, according to this conception, there is no such thing as an extra-organic stimulus. In order to be a stimulus at all, an excitation must already have become a function within the total process of coördination. What has been said regarding the stimulus may also be said concerning the response. It has meaning, it has existence, only within the act of coördination. The mistake usually made in connection with the response is similar to that made in the case of the stimulus. It is conceived as external and overt only. The truth is that the so-called central process to which, exclusively, the *idea* is supposed to correspond is itself the beginning of the *act* of the response. The problem of adjustment is not only the problem of the adjustment of the organism as a whole to an external environment; it is also, and very largely, the problem of the adjustment of the various organs to each other within the organism. That is, the two functions of an organism, stimulation and reaction, suppose each other. We never know that an organism has been stimulated, in the first instance, in fact we do not even know that it is an organism, except through the reaction. The organism is not in certain cases expressive and in certain other cases receptive, but it is active always. Its passive or receptive (sensory) and its active or expressive (motor) aspects can be separated only for convenience of analysis, and even then such a division is of doubtful value. Useful lines of cleavage cut right across such a distinction.

From this point of view the conception of an arc clearly is inadequate. The arc might just as well be a straight line so far as the theory is concerned, though those who hold to this view invariably, though inconsistently, bring the two ends of the arc together in some way. In every organic circuit it is as true that the responsive attitude through the kinæsthetic imagery controls the selection of the stimulus as that the stimulation determines the character of the response. The whole nervous system is a mechanism for such interaction. This is made possible through the accommodatory apparatus of the special sense organs, the muscles concerned in the adaptation of the organ to the external excitation giving rise to the kinæsthetic imagery which is di-

rectly correlated with that received from the motor organs concerned in the response. The only difference is that in the case of the special sense organs the overt motor function of expression has been reduced to the minimum in the development of the race, while the dominant imagery connected therewith has absorbed the whole field of consciousness. The kinæsthetic sensations, though they arise chiefly in connection with the motor organs or muscles, are not for that reason any more motor than sensations from any other part of the body. The only difference is that in the case of the special sense organs the parts are adapted for the reception and transmission of impulses centripetally and the motor parts connected therewith are overshadowed in this development or modified into a special accommodatory apparatus for this same end, while in the muscles the chief function which has developed is that of the transmission centrifugally of the nervous impulses and their mediation into expressive acts while the receptive and centripetal process is greatly overshadowed, being represented in the sense organs of the muscles, tendons and articular surfaces.

A lucid statement of this interaction of stimulus and response has been made by Professor Angell in a paper on 'Reaction-Time' in the PSYCHOLOGICAL REVIEW.[1] Suppose, he says, that we attempt to analyze any typical coördination. Take the case of the response to a signal as simplified in the reaction experiment. Let it be an auditory-hand reaction. Then the process of adjustment or the coördination may be stated in terms of habit and attention as follows: These are Professor Angell's words: "As the reagent receives his instructions for the reaction, he formulates in imagination what he is going to do. This formulation, the getting in mind what he is to do, is his attention to the act. Whatever may be the detail of imagery involved in this formulation, it involves primarily the coördination of two groups of incoming sensations, one from the ear, the other from the hand, started by the operator's descriptions. From this, two distinctions may be drawn: (1) As related to the act of attention, these two sensation groups are its stimuli; and each group is as much stimulus as the other—the sensations from the hand as much as those from the ear. The 'reaction' as meaning the whole act to be performed is not the mere response of the hand to the ear, but the act of attention in coördinating the incoming stimuli from *both* the hand and the ear. Concerning the 'sensory-motor' distinction it follows that, since the stimulus, *i. e.*, the material for the act, lies in these incoming currents from both hand and ear, as related to the whole act, both

[1] May, 1896, III., No. 3, pp. 254–258.

'forms' may be regarded as equally 'sensory' or equally 'motor.' (2) In relation to each other *inside* the act of attention, most discussions of the subject appear to make the ear process merely a stimulus to which the hand adjustment is merely a response. But the question arises, What holds the ear to its work? Why does the reagent maintain his listening attitude? It may be replied that it is 'because he is told to.' But he is not told to listen any more than he is told to move his hand. If the telling suffices in one case it should in the other. Moreover, he is not *merely* to listen, or even to listen just for the click, but to listen for the click *as a pressure signal.* The hand therefore is stimulus as well as response to the ear, and the latter is response as well as stimulus to the hand. Each is both stimulus and response to the other. The distinction of stimulus and response is therefore not one of *content*, the stimulus being identified with the ear, the response with the hand, but one of *function*, and both offices belong equally to each organ. The reason the movement of the hand is so often treated as the *mere* response to the ear as its *mere* stimulus appears to be that the whole act, or 'reaction,' is identified with the movement of the hand. But the entire act is the act of attention in coördinating the two groups of stimuli coming from both hand and ear."

This point of view enables us to understand the significance of Flechsig's contention that the so-called sensory cortical centers are as much motor as they are sensory. It is not necessary to give here an exposition of Flechsig's argument. Statements of it may be found in his 'Gehirn und Seele.'[1] The older view looked upon certain areas as sensory and upon certain others as motor. The error of this view from the physiological and psychological standpoint lies in supposing that the sensory phase of the process which answers to a given coordination corresponds exclusively to impulses transmitted from the special sense organs.

This standpoint gives us also a fundamental basis from which to criticise current theories and classifications of types of aphasia. The old idea that certain forms of aphasia are sensory and others motor was due largely to an inadequate conception of the nature of the organic circuit. There was set up a false antithesis of the sensory and motor processes. The 'moteur' was distinguished from the 'auditif' and 'visuel' by reason of the fact that he had a consciousness of the beginnings of the movements that are necessary in order to express the

[1] P. 65; statements in English will be found in Collins, Faculty of Speech, p. 106 f., and in an article by Barker, *Jour. of Nerv. & Ment. Diseases*, 1897, p. 336 f.: Cf. also Biervliet, *Revue Philosophique*, 1897, p. 122 f.

idea in external form. It was not seen that this so-called motor or kinæsthetic imagery is necessary to the auditory or visual processes as well. It was not seen that it would not be possible for the individual to understand or interpret any sensory process of stimulation if there were not at least an incipient motor discharge. So that the distinction which so many writers on aphasia have made between motor and sensory aphasia as involving disturbance in the expression and in the reception of speech ideas, respectively, however practical it may have been from the clinical standpoint, is certainly not a sound one psychologically.

Aphasia should be classified either entirely from the standpoint of its sensory or entirely from the standpoint of its motor aspect. The two categories should not be mixed. If classified from the sensory standpoint the classification is a psychological one. If classified from the motor standpoint the classification is a physiological one. Of course, in a complete study of the subject, aphasia will be treated from both sides; from the psychological side when we speak of the types of imagery involved, and from the physiological side when we speak of the reactions which are the external expression of this imagery. But in classification for the sake of clearness these two aspects must be kept distinct. If a classification were to be made from the side of the sensations or imagery involved, we should have three main types of aphasia: auditory, visual and kinæsthetic. If a classification were to be made from the side of the reactions involved, we should have two chief forms of aphasia: aphasia proper (aphemia) and agraphia.

The erroneous use of the term 'motor' has led to the neglect of the proper sensory aspect of the kinæsthetic sensations. Biervliet has called attention to this in the paper to which reference has been made.[1] He employs the following illustrations. A child hears and sees us speak. This produces auditory and visual sensations in the child, and these sensations in turn arouse kinæsthetic images either because of an inherited mechanism with paths of least resistance connecting with the kinæsthetic area or because of the associations of the kinæsthetic imagery with similar motor reactions in the past experience of the child. By one or both of these means the child comes to get a kinæsthetic as well as an auditory and visual image of the world. Then the child endeavors to react in a manner which will produce in himself sensations which will correspond to those derived from hearing other persons speak. Here, plainly, the kinæsthetic imagery may be viewed either as sensory or as motor according to whether you view it

[1] *Op. cit.*, 3, 117 f.

from the standpoint of the process of stimulation, the speaking of the other person, or from the standpoint of the process of response, the so-called imitative reaction of the child. Or, take the audience in a theatre while a thrilling situation is being portrayed. Many persons who are not accustomed to self control will show the dynamogenic tendency of the visual and auditory imagery in the altered tonicity of the muscles as revealed by the half-suppressed reflexes which escape them. The kinæsthetic imagery which is involved here may be regarded from either of the two points of view. It may be viewed as imagery aroused by the sense presentation (chiefly in visual and auditory terms), the performance on the stage. That is, it may be viewed as the terminus or consummation of a sensory process. Or it may be viewed as initiating these motor reactions which involuntarily escape the auditor and spectator. Here it is regarded as the commencement of the response. In other words, there is no more reason for regarding the sensations connected with the functioning of the sense organs as sensory than those connected with the functioning of the muscles. On the other hand, the sensations arising from the muscles are no more motor than the sensations arising from the sense organs. The term 'motor' here is really irrelevant. To repeat, the terms 'sensory' and 'motor' properly used are not content but functional distinctions. By this it is meant that the motor processes of the reaction or response have qualitatively no different representation in consciousness from that of the sensory processes. There is no such thing as a motor consciousness distinct from and yet on a par with the ideational and feeling consciousness. All consciousness is motor in one aspect, and all consciousness is sensory in another aspect.

This difference of aspects, when it affects the standpoint and method of science, marks the difference between physiology and psychology. The 'motor' process is a limiting concept in psychology, and the 'sensory' process is a limiting concept in physiology. The motor function is to the psychical attitude in psychology what the categorical function is to the development of the judgment in logic. It marks the limit, the end, of the psychical as psychical. On the other hand, the 'motor' process of function is the working principle of physiology. All study of function in the physiological laboratory is a study of physical expressive reactions. And all determination, all measurements of the process of stimulation is in terms of our possible reactions to such stimuli, whether the unit be a micromillimeter or a pace or a day's journey. That is, here the sensory function is the limiting concept. A given organic circuit is either all sensory or all

motor. It is not part of it sensory, say from the sense organ to some cortical center, and part of it motor, from this center to the peripheral musculature. The whole circuit is sensory when viewed in its function or aspect of mediating a conscious experience. It is all motor when viewed in its function or aspect of mediating an act. That is, from the standpoint of the experience as introspectively felt or sensed by the individual, the whole process is sensory. This is the strictly psychological viewpoint. From the standpoint of the reaction to which this experience leads, a reaction which takes place in the world of space and time and is palpable to other individuals and hence what we call objective or physical, the whole process is motor. This is the strictly physiological viewpoint.

The element of truth in the characterization of the kinæsthetic imagery as 'motor' is this, that the kinæsthetic imagery is in a peculiar sense the fundamental imagery of meaning. It is an accepted fact that the tactile sense is genetically prior to the other senses. Now the kinæsthetic sense is closely related to the tactile sense in origin. This means that, primitively, all other imagery is translated into terms of contact values. This has sometimes been called the 'practical' origin of experience. We first began to know because and for the sake of doing. We learn by doing. Consciousness genetically follows, it does not precede the act. The first imagery to be developed is the tactile and kinæsthetic imagery, and this constitutes the bulk of that vague mélange which is called the empirical ego.

This is the truth in any doctrine of the primacy of the will. Those who defend the existence of a motor consciousness which stands on a par with the feeling and cognitive consciousness ordinarily have these facts in mind when they insist on attention, apperception and will as distinct faculties of the mind. They do not see that all this activity, this motor side of experience, is reported in consciousness solely in terms of feeling and sensation. They do not see that activity is the one thing which, as activity, cannot be brought into consciousness. Activity is always something physical, a play of material forces; it is never psychical. Feeling and sensation are psychical; they are the meaning or significance of that activity, of that interplay of physical energies. What various writers speak of as the ever-present activity of the mind unifying all the life of sensation and feeling is itself, psychologically described, only a congeries of sensations, the kinæsthetic sensations, which differ from others only in this, that they are the invariable accompaniment and condition of the development of all the other types of sensation.

All stimulation, whether unconscious or conscious, sets up in the organism a dynamogenic tendency which can be registered by means of the dynamometer. The way in which the kinæsthetic image tends toward movement does not differ from the manner in which any image tends toward movement. The only difference is in the immediateness with which the various types of image lead to the movement. Every auditory and every visual sensation tends to call out certain responses through the mediation of the kinæsthetic imagery. This intermediate kinæsthetic imagery does not always become conscious. It may serve as a motor cue beneath the threshold. But that it may become conscious is seen in the introspective studies of such writers as Stricker and Dodge, who have analyzed their own processes of internal speech. Vision and hearing (and the same is true, though not so obvious, of smell and taste) are dependent upon the kinæsthetic imagery in a sense in which the kinæsthetic imagery as a whole is not dependent in turn upon them. The kinæsthetic imagery always stands most immediately for the movement because it arises from the incipient state of activity of the organs in which the movement subsequently is to take place in overt form. This is why the imagery rightly is called the fundamental imagery of meaning, since the meaning of a coördination does not come out fully until after the response has been made (at least in idea). With advancing civilization and culture the tactile and kinæsthetic imagery is inhibited, is kept below the threshold, that is, is brought under control by being made automatic, while the dominant imagery of conscious experience comes to be the visual and auditory. The chief difference between the thought of the man on the sense plane and the thought of philosophical reflection is that in the one case the symbols used are in terms of kinæsthetic sensations (the language of gesture,exclamation and interjection), while in the other case the symbols are to a greater degree in terms of visual and auditory sensations. That is, the kinæsthetic imagery is an operative factor in the first case as it is not consciously in the second case. Where it is not possible for the visual and auditory imagery thus to supplant the kinæsthetic, as, for example, in the case of the deaf-blind such as Laura Bridgman or Helen Kellar, the kinæsthetic imagery retains its original character and becomes not merely the fundamental, but the only imagery of meaning. In other words, it is possible to have a pure 'moteur' but not a pure 'visual' or 'auditif.'

To sum up, these considerations certainly bring out the arbitrary nature of the customary distinctions between the 'sensory' and 'motor' aspects of the organic circuit. This distinction, as ordinarily employed,

furnishes us with no psychological differentia at all, because the attempt is made to make the distinction one of content rather than of function. But viewed simply as functional phases of the process of adjustment or coördination, the distinction has a positive value. It is preferable to substitute the term 'kinæsthetic' for the term 'motor' wherever this antithesis is not intended by the use of the latter word.

H. Heath Bawden.

DR. MEYER'S 'ELEMENTS OF A PSYCHOLOGICAL THEORY OF MELODY.'[1]

The search for the basis of music is centuries old; it antedates the search for the philosopher's stone, the Holy Grail, the North Pole; and it is not likely to be given up by the human mind until success is attained, or a reason is found for believing the search to be in vain.

The multitude of books that purport to give 'scientific laws' of music the author declares to be unsound and dogmatic; so the field being clear in the paper whose title is given in the heading Dr. Max Meyer brings out his carefully reasoned new theory; and if he seems to be dogmatic in manner we must remember that this is customary among writers on musical subjects. While the title calls for a psychological theory, the greater part of the paper covers ground belonging equally to the physicist; and I shall review it from the standpoint of the sister science.

I.

The first word to be spoken is one of cordial welcome to one who has shown himself to be an industrious worker, and willing to leave the beaten tracks. For the ultimate explanation of musical phenomena and development must be given by the psychologist; of course he must make large use of the materials furnished by the physicist, ethnologist and historian, but he must add much of his own procuring and combine all in accordance with mental laws. And the next word is one of satisfaction that the author is not ensnared by the pretty theories of the diatonic scale, which has as a matter of fact been consciously followed by only a small part of the men who have concerned themselves with music; in this opposition to the notion that the diatonic scale underlies all music he parts company with his former teacher, Professor Carl Stumpf, some at least of whose numerous writings are known to every scientific student of music.

After the introductory pages the development of the author's theory begins thus: "When we hear successively two tones the vibration rates of which are to each other as 2 : 3, or briefly speaking the tones

[1] Psychological Review, May, 1900, pp. 241–273.

2 and 3, we notice something not describable which I shall call the *relationship* of these tones." "The hearer will observe something else, namely, that after hearing 2 and then 3 he wishes to go back to 2." Generally "no melody that contains 2 can end with another tone but 2." A similar *relationship* is observed between 2 (or powers of 2 including 1) and 5 and 7 and their multiples, but not between higher prime numbers, 11, 13, etc., and the lower ones. So a table is made out containing all the powers and products of these four numbers, 2, 3, 5 and 7 arranged systematically in octaves; the author says "I have found the series to suffice when continued up to 1024" and "I have found no case where higher powers of 5 and 7 are used than 5^3 and 7"; with these limitations, if the products are considered as giving vibration frequencies, the resulting scale has in the octave below 1024 d. v. 29 steps. This is the 'complete musical scale,' and it was embodied in a reed organ which gave all the notes of the table from 64 to 1024 d. v.

Then the author selected a few folk-songs, and several melodies dissevered from their accompaniment, of dates running from Mozart to Wagner, transposed them to the key of C and played them on his organ. Since there are 29 notes to the octave, there is opportunity at each point to select that one of two or three notes which produces the most satisfactory impression, so "it is in most cases very easy to determine what pitch is meant by the composer." Into the discussion of the melodies we must not go, though it is interesting to see how the author attempts to explain the tendency to progress in the melody by the hearer's constant expectation of reaching 2 (*i. e.*, in the familiar nomenclature, the tonic); but this must not be reached prematurely: so on p. 267 he gives to one C the value 63 instead of 64, thus making it a comma and a quarter flatter than ordinary readers would place it. Similarly on p. 256 the unaccented passing note F is called 21 instead of 21⅓ as would be the case in 'just intonation.' But it may be questioned whether it is legitimate to dissociate the melody from the composer's harmony in any of these cases and treat it as if it were based on a non-harmonic scale; while the analysis of folk-songs is free from this difficulty, there is the equally serious one, that the tune as written is often only an approximation to the actual one, being dependent on the prejudices and limitations of the man who noted it down.

II.

In discussing this theory we may notice first the theory in itself, and its presentation, then its relations to some older work. Unfortu-

nately the title is not clear, and in the paper it is not made certain whether the author means to present some elements or all the elements, or whether all melody is to be thought of, or only that of Europeans: but these distinctions are important. However these may be, the theory is based on and logically developed from the statements quoted above: it is significant that these are not put in the usual form of axioms, *i. e.*, impersonally and generically; the wording is 'we notice * * * *relationships*'; and the 'we' in the statements about experimental work appears to be the author alone. In these statements it is the man of harmonic training who speaks; for nothing I think can be more certain historically than that these *relationships* have been unrecognized by most of the men throughout the ages who have concerned themselves about music. The results of such training on students of alien music I have discussed elsewhere[1] and need not go into it here. The author's theory is logically developed as already stated, but only up to a certain point, where it is stopped because of a reason which does not rise above the personal plane; so the scale is not theoretically 'complete,' but at most only sufficient for present needs.

The description of the new organ, by which the theories are to be upset that were current when the music that is quoted was written, is so meager that one hesitates to draw any conclusions from the experiments performed on it: then too the *relationships* noticed between its sounds will almost certainly be much more marked than between the notes of voices or instruments familiar to musical composers; and a little error in tuning or difference in quality, or in the order in which the alternatives are presented to the ear (as Planck found) may determine which of two rival notes shall be chosen. And then the fact that the preferences noted appear to be those of only a single observer, whose æsthetic training and sensibility however high are not stated, prevents our accepting them as valid generally, even for people of harmonic training.

The presentation of the scale would be much clearer if the values of the notes were expressed in terms of the usual piano scale. This is easily done by using the values for the author's 2, 3, 5 and 7 in equally-tempered semitones, viz, octave, 12.00; fifth, 7.02; major third, 3.86; natural seventh, 9.69 E. S.; add these values together where the author's numbers are multiplied, casting out the twelves; *e. g.*, $63 = 7 + 3^2$ or $9.69 + 7.02 + 7.02 = 23.73 = 11.73$ E. S., which is 0.27 E. S. flatter than the octave. The comma is 0.22 E. S.

[1] *Science*, New York, Feb. 9, 1900.

III.

Now compare this new scale and the assumptions and conclusions associated with it with the work of former students. The Greek theorists were content to work with 2 and 3, their products and powers. Zarlino broadened the foundations, as the author points out, by introducing 5, and therefore making more use of 6 than the Greeks did. Poole and others have insisted on adding 7, though it upsets the diatonic scale. This question of the 7 is not one of mathematical jugglery or of musical interpretation by the hearer, but simply this: did the composer mean to use at a certain point a natural seventh (written (7), having a pitch 0.31 E. S. below Bb in the scale of C), and score the passage for an instrument that could give it (horn, violin, etc.)? Here is one authoritative answer: Gevaert, Director of the Brussels Conservatory, says in the Annuaire of the institution for 1883 (about p. 200), that the natural seventh is used by modern composers, but not the natural eleventh or thirteenth. But admitting this, it is questionable whether the older composers quoted by Dr. Meyer had the command of this new note. On the other hand, the argument from use, which justifies admitting (7) to the 'complete scale,' requires also the admission of many notes which the author excludes (see Gevaert's paper). Most conspicuous is the absence of the correct fourth, 4.98 E. S., *i. e.* the fifth taken downwards, and the notes of the series built on fourths, Bb, Eb, etc.; his substitutes for F are (21) 4.71 E. S., and (675) 4.78 E. S. Only the most conclusive reasons can justify this outlawing of notes which almost everywhere, except in China, have belonged to the aristocracy of the scale; yet no reasons are given.

But Dr. Meyer is not alone in thus treating the scale. About twenty years ago Dr. Ivan Zoch[1] was impressed with the fact that most Slavic folk-songs cannot be accompanied on the piano. This led him to study the collections of songs and the native instruments, especially the Tumbura, and finally to develop a scale therefrom. He says "The natural scale must lie in natural numbers" and "all scales in which there is a greater prime than 7 appear unmusical." So the basis is

1, 3, 5, 7, 9, 15, 21, 25, 27, (35), 45, (63), 75, (81, 105, 125), 135, etc.

All melodies as actually sung he found could be correctly represented on a 12-tone scale with the above numbers, omitting those in parentheses, divided by the power of 2 next smaller; his scale in Dr. Meyer's notation with the musical intervals added is as follows:

[1] Carl's Repertorium der Experimental Physik, 18, 748–764 (1882).

2	135	9	75	5	21	45	3	25	27	7	15	2	
0	0.92	2.04	2.74	3.86	4.71	5.90	7.02	7.72	9.06	9.69	10.88	12.00	E.S.

This scale was carefully laid out on a monochord and a piano tuned therefrom. Into Dr. Zoch's classification of the shorter scales selected out of this we cannot enter. This attempt to find a scale in which the strange and powerful Slavic folk-songs could be expressed seemed so promising that I wrote to the eminent acoustician Dr. Dvorak, of Prague, about it; his reply was the disappointing one that the theory was not proved.

Underlying these and many other studies of the scale there seems often to be an assumption that the world has but one scale and one body of musical rules, though perhaps no one expresses it so boldly except in speaking of music as 'a universal language.' It may not be amiss, therefore, to clear away any misconception by some quotations. The first is from an article[1] on Wagner's methods: "His other innovation which is not even yet acceptable to all ears, is to employ the chromatic scale of twelve equal semitones as a basis for melody instead of the diatonic scale. The whole of the music of Tristan and Isolde would be impossible under the old laws. I need only quote one example and by no means an extreme one, of a passage impossible to sing or to listen to in anything but strictly equal temperament [quoting the duett, 'Blissful Dreams']. It is a constant wonder to me how singers trained upon diatonic scales can sing this and many still stranger passages with anything approaching bearable intonation." A little earlier than the date of this article Professor Donkin,[2] of Oxford, 'who was an accomplished musician and had a profound theoretical knowledge of the science of music,' and wrote on Greek music for Smith's Dictionary of Antiquities, said "The structure of modern music is founded on the possibility of educating the ear not merely to tolerate or ignore, but even in some degree to take pleasure in slight deviations from the perfection of the diatonic scale."

When we go to the study of Oriental scales the assumption of a single law is found to be positively contradicted. We know by what rules many scales have been formed, and the idea of *relationship*, in the sense of the author, is often if not always wanting. The principle which gives unity to the composition is something other than the harmonic *relationship* which tacitly underlies his numerical work. Even the demand for a close on the tonic, on which he lays so much stress (p. 257) is not commonly met with in Hindu music; though the final

[1] *Musical Times*, London, October, 1896, p. 652.

[2] Acoustics, Oxford, 1870, pp. 25, v.

may be C 'accidentals' (if Dr. Meyer will pardon the word for want of a better one) have often destroyed the feeling that the tune is in the key of C; and it is rare to find a Hindu tune that seems to ordinary musicians to end right.

IV.

In conclusion, I would like to suggest to any student who would join in this search for the basis (or the bases) of music that the greatest difficulty in the study is to get into intelligent sympathy with men of alien thought, and understand both what they did and their reason for doing it; for it is almost impossible to avoid coloring the facts by our own training and associations, or those of the observers on whom we must depend. This influence of association and habituation deserves investigation by psychologists. If the student would learn of non-harmonic scales, Mr. Ellis' papers[1] detailing his and Mr. Hipkins' experiments is still the best introduction; his condensation of it for the Appendix to Helmholtz is a dry skeleton. If the student would know of the mediæval scales many papers in the Vierteljahrsschrift für Musikwissenschaft will give him light. If he would see how pugnacious and discordant the European theorists have been, and cultivate his sense for the historic elements of the problem, he should take such a bird's eye view as is given under 'Harmoniesystem' in Mendel's Musik. Lexicon; and if he would learn of the scales actually rendered by European public performers and how to train his ear for observing them, he may be commended to the admirable papers in the Vierteljahrsschrift for 1893, by Röntgen and Dr. Planck. Of course he will soon see that in any theory of music there are two questions involved which in this brief discussion we have not discriminated as carefully as Dr. Meyer did, viz: on what principle or principles are certain notes selected to form a scale, and by what principles, physical, psychological, æsthetic, historical, etc., are these notes rearranged to form tunes.

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WASHINGTON, D. C.

[1] *Journal of The Society of Arts*, London, 1885.

PSYCHOLOGICAL LITERATURE.

Des Indes à la Planète Mars. Étude sur un cas de somnambulisme avec glossolalie. Par TH. FLOURNOY. Paris, F. Alcan; Genéve, Ch. Eggimann & Cie. 1900. Large 8°. Pp. 420.

M. Flournoy, professor of psychology at the University of Geneva, presents a remarkable case of mental automatism or subconscious personality, which fully realizes the expectations aroused by the suggestive title, 'From India to the Planet Mars'; and yet, one must hasten to say, the value of the book lies in the success with which the various phases of these 'mediumistic' phenomena have been described and traced to natural and tangible starting points. While the medium and her friends are firm in the belief of spirit control as the only satisfactory explanation of the phenomena, M. Flournoy is thoroughly opposed to any such hypothesis and finds it not only unnecessary but also inconsistent with the facts and inadequate.

The story is a complicated one. The medium in the narrative goes by the name of Helen Smith. Her father was a merchant, a Hungarian by birth, and is described as an active, enterprising, matter-of-fact man and a good linguist; though quite hostile to 'spiritualistic' notions, he was gradually won over to them by his daughter's mediumship. Her mother, born in Geneva, has always been markedly predisposed to spiritualistic phenomena of all kinds, has had 'psychic experiences' of her own, and is also involved as narrator or witness in several of the less clear and less credible phenomena of Helen's mediumship. Helen herself is described as an attractive woman of about thirty years of age, intelligent and frank; she is of good physical and mental health, presents none of the recognizable stigmata of nervous instability, if we except a six-month period of general weakness, and her mediumistic tendencies; and resents strongly the imputation of being abnormal in any respect. At the age of fifteen she became an apprentice in a large business house and has earned her living as a trusted and capable employee. She has refused to allow any photograph of herself to accompany the volume, but has consented to its publication in spite of her radical disbelief in the explanations offered. During her girlhood she was given to day-dreaming, experienced

hallucinations and unusual warnings, and was as she still is of a highly sensitive, nervous and imaginative temperament. She regarded herself as a strange and unusual person, who was in a way out of place in the everyday existence about her, and she was ever ready, though often with fear and trembling, to perceive in unusual happenings the confirmations of her imaginative creations. Adolescence brought about a consummation of many of these tendencies, in the way of more positive hallucinations, momentary lapses of consciousness and sporadic instances of automatic or 'somnambulic' actions. M. Flournoy regards it as likely that these excursions in an unreal world and the tendencies to automatic expression would have disappeared normally and naturally amid the sterner realities of life (for this form of coquetting with castles in Spain and seriously mystic occupation with the less humdrum world of one's imagination is not a rare trait of childhood), were it not for her introduction to the manifestations of spiritualism. These became both rain and sunshine to the tender sprouts of her subconscious fancy and developed them into a tropical luxuriance of automatic manifestations. Table turning and rapping out of messages she accomplished at once, while a slight suggestion on the part of the 'circle' induced automatic writing and clairvoyant visions. These began early in 1892 and continued without any unusual features until the spring of 1895, when, partially under the incentive of the interested and professional presence of M. Flournoy, the 'polymorphous' automatisms of Hindu and Martian blossomed forth.

Mlle. Smith, in her present séances, enters into a trance, or rather into one of several forms of trance, the general reality of which is attested by physiological changes of breathing and attitude, by the presence of abnormalities of sensibility and movement, and by marked psychologic characteristics similar in every way to those of the hypnotic trance. Her appreciation of her surroundings, her remembrance of her trance-doings on return to her normal state, vary in the several trances; several different impersonations or trance conditions may occur in the same setting, and the most remarkable phenomena seem associated with the deepest disturbance of consciousness. Her general guide or spirit-control is one 'Leopold,' who enters partially into all her automatic 'cycles' and into her daily life. She not rarely sees him, or hears his voice; he has indicated the whereabouts of hidden articles, warned her against impending disaster, prescribed remedies for the sick, and in particular directed Helen as to what she may and may not do both in ordinary worldly and in 'psychic' situations. 'Leopold' has been a great help and also something of a hindrance

to the investigations. He alone is in touch with the subconscious strata of Helen's mental storehouse, and can by suitable suggestion be made to yield information which the normal Helen is unable to give; but at crucial points he too professes ignorance, and pronounces *licet* and *non-licet* upon attempts to bring to light hidden sources of 'spirit-revealed' knowledge. Now this factotum and mentor, Leopold, is really the disembodied spirit of Joseph Balsamo, better known as Count Cagliostro, who departed this life with a somewhat shattered reputation in 1795. Leopold seems to personify Balsamo mainly when Helen passes into what M. Flournoy calls her 'royal cycle,' in which she becomes Marie Antoinette and Balsamo her '*cher sorcier*' and devoted admirer. While the unfortunate queen seems in many ways the favorite character of Mlle. Smith's automatic repertoire, and while she assumes the part with superb histrionic realism of attitude and manner; and spends entire evenings as the queen, and partakes an actual dinner which she eats with royal appetite,[1] entertaining her real, but to her transformed, guests, gracious to her favorites and queenly to all; yet this is but the spontaneous exuberance of an imaginative creation the materials for which are readily accessible to her normal self, and many details of which have been traced to an engraving accompanying Dumas' account of Balsamo. We shall, therefore, follow M. Flournoy to India and to Mars.

The Martian cycle seems to have sprouted from a chance suggestion of one of the sitters, a M. Lemâitre, that it would be interesting to know what was going on on the planet Mars, and the further elaboration of the topic after the manner of Flammarion. This notion, 'caught on the wing,' made a great impression on Mlle. Smith's subconscious automatism, and in one of her subsequent clairvoyant visions she seems to be floating away into space and the table spells out "Lemâïtre, ca que tu desirais tant!", and then she arrives at her destination which the table announces to be Mars. At this same séance she also brought messages to an old lady from her dead son Alexis Mirel, who reappears in another incarnation on Mars as Esenale. Then come descriptions of Martian houses, scenery and peoples, of customs and doings, and a bit of its fauna and flora. These are all fanciful enough and are evidently designed to be as oddly different

[1] It is interesting to note that Miss Smith has no recollection upon awakening of having eaten dinner; likewise that while apparently insensible to her surroundings she nevertheless hears the 'asides' spoken by members of her company. After dinner 'Marie Antoinette' lit a cigarette, which action was commented upon as unqueenlike by her court and was never afterwards repeated.

from terrestrial conditions as may be. There is an intermediate condition in which Mlle. Smith can be induced to use pencil and brush and yet can receive by suggestion this visualized Martian scenery; in this way we have quite a collection of illustrations of things visible upon Mars. They are not particularly interesting. The landscapes and houses are rather Japanese, or vaguely oriental; the occasional specimens of a plant and animal are unusual combinations of familiar vegetable and animal qualities, not nearly so droll as those of Edward Lear. The one really remarkable feature of the Martian epos came only after a long period of incubation or subliminal preparation; this was the Martian language. In its fullest development it included the hearing of words in this strange tongue, speaking it, seeing it visualized in space and, best of all, writing it when the medium was completely entranced and personating a Martian. However none of these processes ever appeared as fluent, extensive or completely spontaneous; yet we have short but significant and consistent messages in a wholly fictitious and strange looking alphabet. Here is one of the messages reduced to Roman characters, and its French equivalent:

Astané bounié zé buzi ti di triné nâmi ni ti di umêzé sé ï miré bi tarvini,

Astané cherche le moyen de te parler beaucoup et de te faire comprendre son langage.

But how is the French equivalent known? Through Leopold, who vouchsafed a talismanic word and procedure by which the entranced medium could be induced to translate. The messages do not transcend the familiar mediocrity of spiritualistic circles, but their form is certainly a marvellous example of subliminal creative imagination if we are willing with M. Flournoy to accept them as such. M. Flournoy's analysis of the language is most minute, and he pronounces it an 'infantile' production modelled closely after the French, the only language which Mlle. Smith knows. Its syntax and the arrangement of words are absolutely identical with those of French; the vocabulary is made as bizarre as may be, but it is possible in many cases to recognize the source of the invention. In brief the noteworthy point is the holding in mind of the visual signs and the phonetic equivalent of these signs, and of their combination into words, at least sufficiently to hear, see and write brief messages; the imaginary Martian setting and corroborative, 'details added to give verisimilitude to an otherwise improbable tale,' are creditable to a subordinate personality, but they do not arouse the admiration evoked by the auditory and visual memory feats.

The Hindu cycle is even more complicated, and its element of mystery remains as yet an unsolved problem. In this, Mlle. Smith appears as the daughter of an Arab Sheik, whom she leaves to become, under the name of Simandini, the eleventh wife of Prince Sivrouka Nayaca, whose present incarnation is none other than M. Flournoy himself. This Sivrouka reigned over Kanara and built in 1401 the fortress of Tchandraguiri. Other characters are a faithful servant Adèl, a small ape Mitidja and a fakir Kanga who is no other than the Astané of the Martian world. Upon this foundation there is again elaborated a complex drama too intricate to be here unfolded. The first query is whence this knowledge on the part of Mlle. Smith who, as was expected, in her normal condition knows nothing of it at all. M. Flournoy consulted specialists, but all reported that there was no such person as Sivrouka known to history. What was his surprise on finding in volume one of a history of India by De Marlès published in 1828, the entire information mediumistically revealed by Helen. But the good story must again be spoiled, for it appears that De Marlès' authority is well nigh worthless and that the facts he relates are probably far from historically correct. None the less the question as to how this information reached Mlle. Smith (who normally knows nothing of De Marlès) remains a mystery; Leopold professes ignorance and he alone knows. But, as before, the most remarkable feature is the linguistic one, for sporadic utterances of 'Hindu' are taken down from the mouth of Simandini (alias Mlle. Smith). When such words as can be recognized (which by the way Leopold will not translate or write, professing ignorance of Oriental tongues) are recorded, they are found to have close resemblance to Sanskrit words. The resemblance is only partial and crude, and M. Flournoy concludes that a casual acquaintance with a Sanskrit grammar is sufficient to account for the interspersing of a few genuine words amidst much that is unrecognizable. However, no such knowledge can be traced to Mlle. Smith and every reader may form what hypothesis he choses until the future reveals the mystery. That the personality who composes the Hindu drama and incorporates the Sanskrit words is the same as that of the composer of the Martian and the royal cycles, M. Flournoy convincingly shows; and this is the main point.

To complete the tale, something should be said of other supernormal happenings which Mlle. Smith has experienced and which M. Flournoy accepts with a rather uncritical estimate of the reliability of his evidence, which is for him second-hand. His ready acceptance of telepathy and 'telekinesis' reveals a bias in favor of these hypotheses

which seems out of harmony with the remainder of his analyses. There are some curious personal experiences of Mlle. Smith which go to show the far-reaching character and the remarkable memory of her subliminal self.

Studies of individual psychology are rarely satisfactory, whether they relate to usual or unusual persons; yet they undoubtedly have their value. The general shaping influences of events and tendencies can rarely be applied to the individual without many 'possibles' and 'probables' and 'perhaps.' The specific and particular shape which mental processes and products assume may not be a matter of pure chance, but it is the result of so many, so trivial and so complex influences that it must be treated in much the same way. And so the psychology of Mlle. Smith is most instructive when stress is laid upon the more general features of her subliminal mentality, and not upon the concrete and detailed appearances. It is idle to speculate why her Martian alphabet assumed just the form it has; it is sufficient to note its general origin. Viewed in this way, and granting the complete truthfulness of the phenomena as described, we have a truly classical instance of the psychological comprehensiveness of the automatic self in exceptional cases. M. Flournoy has thus accomplished a valuable task and has presented his material with unusual skill. The dangers of a false interpretation of this 'psychic' tale are many, and are certain to mislead many whose interest in and powers of comprehension of cases of this kind are not equally developed. It is a pity, too, that considerations of courtesy and social obligations make it impossible to fill certain interesting gaps in the narrative; yet the delineation, on the whole, is intelligible enough as it is. It seems probable that when a sufficient number of these cases have been collected, that their general nature and law-abiding character will be understood, that a rational group of associations may be clustered about the term 'mediumistic,' and that the temptation to magnify the doings of some one of this class into a bundle of miracles will gradually pass away—all of which would be welcome consummations for the progress of Psychology.

JOSEPH JASTROW.

Primitive Love and Love Stories. By HENRY T. FINCK. New York, Chas. Scribner's Sons. Pp. 851.

This work is a compilation, rather popular in method and scope, in support of the author's thesis announced in his previous work, 'Romantic Love and Personal Beauty,' as to the very late evolution of romantic love. The first half of the work is mainly given to a discus-

sion of the ingredients of love." "It has indeed an egotistic side, including the ingredients I have called individual preference, monopolism, jealousy, coyness, hyperbole, mixed moods and pride; and it is not a mere accident that these also are the seven features which may be found in sensual love too; for sensuality and selfishness are twins. But the later and more essential characteristics of romantic love are the altruistic and supersensual traits—sympathy, affection, gallantry, self-sacrifice, adoration, purity and admiration of personal beauty." The discussion of personal beauty is an onslaught on Darwin's and Westermarck's views as to sexual selection. The latter half of the book gives illustrations of primitive love among African, Australian, Polynesian, American, Hindoo, Hebrew and Greek peoples, and concludes with a chapter on the utility and future of love in which the author follows Schopenhauer. The work contains a good bibliography and index.

It hardly needs a large volume to prove that the highest, most altruistic relation of the sexes is very late, although we believe germs and occasional manifestations occur quite low down in human life, for even in the quotations made on primitive love, the authors often note that unselfish affection is not entirely unknown, but merely very rarely known. We judge that Mr. Finck would make romantic love as late as sense of the sublime, a position which even upon his own evidence is doubtful, and the evidence itself is for the most part of slight value, being the impressions of travellers and foreigners, and there being no thorough study of a single case. The work, though readable and in a measure suggestive, is much too cursory and comprehensive to have any great value, and being conceived as a personal special plea it is not carried out in the cautious, candid, scientific spirit.

HIRAM M. STANLEY.

LAKE FOREST, ILL.

VISION.

Ueber die stroboskopischen Erscheinungen. ERNST DÜRR. Philosophische Studien. Bd. XV., Hft. 4. Pp. 501–523. 1900.

The article is a report of three series of experiments which developed out of a study of intermittent stimulation and stroboscopic phenomena in collaboration with Dr. Marbe.

The first series is no more than the casual observation, by Dr. Marbe and the author, that a disk of six sectors, alternately black and white, fused at a lesser velocity than a disk with an equal number of sectors, and the same average intensity, on which appeared a double

series of black, gray and white sectors. This phenomenon is found to be entirely congruent with Marbe's theory of 'characteristic effect groups'; and is the basis for the generalization of a new determinate of the fusion of intermittent stimuli, viz, "An increase in the number of different stimuli is unfavorable to fusion."

The author assumes that the only changes effected by the introduction of gray into the black, white series were a reduced difference of intensity between the different sectors, and an increase in the number of different stimuli. The first is favorable to fusion; the hindrance must consequently be due to the second. The general delay in the recurrence of all the stimuli is overlooked entirely; though a little further experimentation would have sufficed to show that it is this, rather than a mere increase in the number, which hinders the fusion.

The second series demonstrates that fusion in stationary stroboscopic phenomena is retarded by the same conditions that retard the fusion of sectors of revolving discs, including the conditions produced by increasing the number of stimuli. The author interprets this as 'an experimental confirmation' of Marbe's general contention, that the stroboscopic phenomena, so far as they do not produce movement, may be explained completely by the laws of intermittent stimulation.

The third and most important series was aimed at discovering the cause of our inability to detect the absence of even a considerable number of phases of a stroboscopic movement. This is neither the number of phases omitted, nor the spacial extent of the omitted movements, within certain limits, but is rather the time interval of the interruption, which shows a relatively constant critical period of something over .2″, so long as the eye is free to follow the movements naturally. If some constant point of the figures is fixated, the author finds, with Marbe, that a very small interruption is noticed.

The author's explanation is as follows: When the eye follows the movement, the various phases excite approximately the same parts of the retina; and the interruption of the phases can only be noticed when it is long enough for the after-image of one stimulation sensibly to diminish before the next stimulation occurs.

This conclusion is further supported by reference to the circular movement of a point of light in a dark room. So long as the eye followed the light freely, a 'critical period' of considerable duration was found, largely independent of the arc of interruption. When however a stationary bit of phosphorus was fixated, very minute arcs of interruption were noticed entirely independent of their duration.

The phenomenon has thus a simple peripheral, rather than a central

cause, as Marbe *et al.* had held; and the author believes that the stroboscopic phenomena of movement have ceased to occupy a peculiar position and may be entirely explained by the laws of intermittent retinal stimulation, and eye movements (p. 517).

Aside from the analogy of after-images of motion, etc., it must appear as a difficulty in Dürr's explanation, that the 'critical period' of interruption for a moving point of light was about one-tenth that for stroboscopic phenomena; while in general it is hardly probable that the after-image of a moderate stimulus would prevent the recognition of an interruption approximately .2″ long. Furthermore the actual eye movements, which I have been able to detect in a series of simple and large figured tests have been altogether too few and irregular to warrant the importance they are given in the hypothesis. Unfortunately the author assumed rather than investigated them.

RAYMOND DODGE.

WESLEYAN UNIVERSITY.

On the Psychology and Physiology of Reading. I. By EDMUND B. HUEY. The American Journal of Psychology, XI. Pp. 283–302.

The article is a partial report, chiefly physiological, containing a most valuable contribution to our knowledge of the eye movements in reading.

The Delabarre eye-cup and a delicate recording pointer, with time-recording attachments, gave kymographic tracings, exhibiting with great distinctness the number and relative positions of reading pauses, together with the extent and velocity of the eye movements which separated them.

The most noteworthy results are the time values of the various processes. The relatively constant forward movements average 41–48 σ. The return sweeps have a much higher velocity, averaging only 52–68 σ, although the arcs of movement are at least three times as great as the forward movements. The reading pauses average 183–191 σ in those series made under satisfactory experimental conditions.

These values are unquestionably far more reliable than any yet obtained by the aid of visual methods. The reviewer doubts, however, if we can accept them without some correction. It is possible that the extra work the eye performs in moving the long pointer with short leverage may not essentially delay it. The assumption certainly demands experimental investigation.

The author's eye reaction, after practice (181.7 σ), approximates very closely my later measurements by the visual method, which were evidently not at hand when the article was written.

The concluding section reports the extent of the reading field in successive 15 σ exposures of connected discourse.

RAYMOND DODGE.

Zur Hypothese der Sehstoffe und Grundfarben. By Dr. ADOLPH STÖHR. Leipzig u. Wien: Deuticke. 1898. Pp. 103.

In the hypothesis which is brought forward in this paper the author assumes several visual substances, and, in the end-apparatus of the optic nerve, ultra-microscopic visual elements, or corpuscles, which are excited, under proper conditions, by the visual substances. It is further supposed that the direct action of light upon the visual corpuscles is already sufficient to set them into molecular vibration, and that the effect of the visual substance is simply to modify and to strengthen this vibration; the object of this duplex supposition is to combine in one a photo-physical hypothesis and a photo-chemical one, —the physical action of light on the corpuscles is common to all animals that have the power of vision, while the photo-chemical portion of the hypothesis applies only in the case of those animals which possess the visual substances. In the highest animals these substances are three in number and they give rise to the elementary color sensations; a single visual corpuscle is excited in three different ways by the three visual substances, and if by all of them at once in a resultant 'unitary' fashion. There is no difference in the function of the rods and cones as regards the quality of the sensation furnished, but the rods are more serviceable in a faint illumination and the cones in a high light.

To particularize farther, the visual elements (corpuscles, or as they are better called later, threads) consist of minute fibers each thickly strewn over its entire surface with projecting flat plates, four in a given plane, and composed of material of four different sorts, fitted to respond to four different sorts of stimulus and to furnish three primary color-sensations together with the sensation of white. [This hypothetical structure corresponds well with that lately made out by Patten in the rods and cones of certain animals.] The most characteristic part of the theory is that these little plates are set into molecular vibration in the first instance by the direct action of light (all alike in this case, and attended by the white sensation), but in the second instance this vibration is added to by the (respectively synchronous) molecular vibrations of the three photo-chemical substances, when severally produced by light of different periodicity. It is then neither the production nor the destruction of a photo-chemical substance that is effective in

exciting the nerve-terminals, though both of these processes are brought about by the action of light; but it is during its relatively brief period of existence that a given substance (in degree corresponding to the amount of it which is present) acts upon the particular color-plate with which it is in harmony. It is remarkable how many hitherto disconnected phenomena the author has been able to bring under common rubrics by means of this conception (which is not in itself by any means too far-fetched) the different varieties of color-blindness, to mention one instance, and also color-blindness and the change of hue dependent upon changes of intensity of light are brought into a single category. In fact, this theory is an intellectual achievement of wonderful ingenuity; it is for the reader a source of keen logical enjoyment to become acquainted with it. A surprisingly large number of the characteristics of the light and color manifold it has given fitting physiological representation to. The author has built up a most ingenious and complicated system of fortifications, so to speak, in which one feature after another, bastion, curtain, gorge, is so constructed as to be, not only in absolute character but also in relative interdependence, in a close and beautiful one-to-one correspondence with the vast array of experimental fact to be accounted for. It gives a norm, and one very difficult to attain to, of what ought to be undertaken by every theory of the qualitative visual sense. And the explanations given by the author are far superior, in the great majority of cases, to those which one is accustomed to in color-theory. They are explanations in the true sense of the word—viz, a physiological conception which is set up for the purpose of explaining one phenomenon is found to have either a necessary or at least a natural consequence by which it also takes account of another phenomenon. Readers of Hering are too much accustomed to hearing it said that a phenomenon has been 'explained' by his theory when it has merely been shown that it is not incompatible with it. There is need of a pair of newly defined words by which to accentuate this distinction.

But unfortunately, notwithstanding the really wonderful amount of acumen that has been expended upon it, and the success that it achieves in the correlation of widely different facts, it seems to the present reviewer that the theory fails to meet the first and fundamental psychophysical requirement of a theory—it is not made sufficiently clear that the resultant of the color-processes is in fact—in physiology—what it is stated to be, a 'unitary' process. The main thing to be explained in a color-theory is, of course, why two fundamental colors when mixed bear still the trace of their origin, while three (or four)

do not. Another objection is that the theory is not sufficiently evolutionary—it does not appear why the three plates which mediate color in the higher animals have already in the lower a distinctive structure which is unnecessary for the purpose which they serve. This is a doctrine of preformation overdone. Dr. Stöhr says that there are a dozen other physiological conceptions which could equally well be worked out as a basis for the comprehension of color-phenomena. It is to be hoped that he will try his skill at some others. Meantime any additions to the subject which may have the effect of weakening the assumed exhaustiveness of the Helmholtz and Hering doctrines are to be welcomed with enthusiasm. *Science* once had an admirable article on the desirableness of keeping the mind evenly balanced (or proportionately balanced), when certainty cannot be attained, between multiple hypotheses. Every time it is shown how much can be accomplished in the way of the correlation and the rendering comprehensible of fact by means of a happy conception, even though a known physiological basis is wanting, the greater is the incentive furnished to the anatomist to carry forward his part of the problem.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

Arbeit und Rhythmus. By KARL BUECHER. Zweite vermehrte Auflage. Leipzig, Teubner, 1899, 412 S. M. 6.

The second edition of this important work contains many additions in the form of working songs from the different quarters of the globe, descriptions of primitive forms of work and many acute remarks of deep insight. In a field where speculation was at home, where the gray barbarian was measured by the ideas and standards of the Christian adult, Buecher has introduced an empirical, scientific method with pleasing, not to say astonishing results. The theories of Aristotle and of other speculative writers must give way to the wealth of material supporting the hypothesis, or shall I say the description, of Buecher in reference to the origin of music, poetry and the dance. It seems natural that a political economist should discover the very intimate relations these functions bear to work. Ferrero has ably shown the causal relationship between work and morality; Buecher extends the relationship to song, music and the dance. In the lower layers of primitive culture work, song and the dance form a trinity in unity. They were mutually complementary in the early crude economy. The enormous labor tasks of primitive men and women, devoid of nearly all the time- and labor-saving instruments which make civilization pos-

sible to-day, can hardly be understood to-day. The hollowing of a log canoe, often with crude stone implements, took so long a time that the wood began often to rot before it was finally finished. The noble savage had to work. But like the child he had an innate dislike for work. He was almost epileptic in his passionate impulsiveness. Savagery and civilization are distinguished in nothing more than in this that the one possesses persistency of economic purpose and habits of continuous labor and the other does not. The Elmira Reformatory Reports show incidentally but convincingly that crime and habits of idleness have a close causal connection. Perhaps the greatest of all pedagogical problems is the formation in the child and in society of habits of persistent industry—to evolve from a chaos of impulses an organized self-directive system of habits engaged in public service.

All those tasks of the savage which could be accomplished with an accompaniment of rhythmic movements of the limbs, and by a rise and fall or rhythm of the voice—and nearly all the tasks of the savage can be included in this list—became easier, more habitual and often a matter of play. Compare the similar phenomena of childhood. There is a like inconstancy of purpose, the same lack of patience and endurance, the same inclination to give themselves over to ever new sensations. It is a long journey from fanciful humor and caprice to stern duty, obedience and obligation. By rhythmic movement, however, habit and automatic activity are evolved. There is an economic saving in the expenditure of energy. Tone-rhythm will be an additional aid and guidance. Rhythmic work in common, especially with the aid of tone-rhythm, will make of a body of men engaged on a common piece of work one single machine. The whole process results in an increase of productiveness and betterment of quality.

Each kind of work has its own kind of music and rhythmic movement. Singing and dancing evolved out of the economic conditions of early primitive methods of work. Work, play, singing, poetry and dancing were not separate provinces, but were necessary parts of an original economic unity. The singing and poetry are the accessories of the rhythmic body movements and were necessarily adjusted to them, often suffering severely in consequence of the enforced subjection. Dancing arose as a continuation of the rhythmic play-work movements. Nowadays they are the beginnings or endings of the harvests and other such activities. The musical instruments of primitive culture are well known to be at least in the majority of cases transformed working instruments.

The work is filled with illustrations and thus appeals convincingly

to the reader. That there are other causes, especially the erotic instinct, at work in the origin of these æsthetic qualities is not denied, but at the same time is not duly emphasized. The origin of the lyric and the drama is lightly but suggestively treated. This is another contribution showing that the so-called æsthetic arts have had a reason for their being on account of their utility. Henle's essay, *Ueber die Grazie* (Anthropologische Vortraege) also bears witness to the truth of the above statements. He remarks that those movements are graceful which attain their end with the least expenditure of means.

ARTHUR ALLIN.

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Two Cases of Synæsthesia. By GUY MONTROSE WHIPPLE, A.B. [From the Laboratory of Cornell University.] Amer. Jour. Psy., Vol. XI., No. 3 (April, 1900), pp. 377–404.

Mr. Whipple has found two good cases of colored hearing, in one of which the colors seem to have a real objectivity, and in the other there is a feature, considered by the investigator to be unique in the history of synæsthesia, viz: a phonism aroused by pain.

The two subjects, *M.*, a young woman student in the Cornell department of psychology, and *R.*, an undergraduate in the college, were subjected to similar series of experiments. The retinal colors were first carefully observed for 10 minutes and described. Then the photisms resulting from I. (*a*) simple noise and tone, (*b*) single tuning forks, (*c*) tuning-fork fusions, (*d*) piano compound clangs, (*e*) tones of church organ (various stops), (*f*) successive fork tones, (*g*) continuous tone change, (*h*) musical pieces on piano, phonograph and church organ; and II. (*a*) tastes (sweet, acid, salt and bitter), and (*b*) smell. Other synæsthetic phenomena such as colors and other characteristics of capital letters were observed. The photisms were projected, also, upon black, and colored, screens. No photisms could be produced by merely *thinking* the music. With both cases the relative position and shape of the colors were noted.

In the case of *M.* the violet end of the spectrum prevailed in most of the photism, while with *R.* red was a prominent color. The subjects projected the photisms upon a chart of colors arranged to show hue and saturation in slight gradations. The chart color that 'matched' in all cases was recorded and this method was used in all the experiments. With *M.*, the colors were frequently of highest saturation and luminous, the saturation being less with piano clangs than with fork tones and the clangs being less pleasant. Organ tone colors were clearer than those of piano clangs but paler than fork photisms.

In the case of *R.* the colors, while not always bright enough to project, were both definite and constant. The pitch and quality of mental auditions aroused by pain were for blunt pressures from a^1 to f^1 and for sharp pressures from b^1 to e^2. One algometer pressure of 2.1 kg. produced c^4.

It is to be hoped that the subjects will continue to be available for systematic research; and that there may be, later, enough experiments to warrant some generalization of results; which Mr. Whipple is at present careful to avoid.

WILFRID LAY.

EXPERIMENTAL.

Ueber die Eigenschaften der Schrift bei Gesunden. Von AUGUST DIEHL. Mit einer Figur im Text, Psychologische Arbeiten von EMIL KRAEPELIN, Dritter Band, 1 Heft, s. 1–61, 1899.

The aim of this research is, from an investigation of the writing of normal individuals of a given class of persons, to develop a basis for the interpretation of the writing of abnormal types of the same level of education. The specific problems guiding the investigation were inquiries, first, in general, as to the mutual relations which exist between the various factors involved in the writing process (extent of stroke, rate, pressure, time occupied in the actual inscription, time consumed by pauses), and, secondly, and more specifically, the effects of various conditioning influences such as practice, diversion, habit, variation in the emotional state, fatigue, etc., and especially to determine the results which followed from making the writing more difficult in various ways. To this latter end, the Arabic figures 1 to 10, were required to be written (1) carefully, the ordinary size, (2) as small as possible, (3) in the way easiest and most natural to the subject, (4) in the reverse order, from 10 to 1. The method followed was essentially the same as that of Gross in his 'Untersuchungen über die Schrift Gesunder und Geisteskranker' published in Vol. II. of the same journal. The chief improvement on the side of apparatus is his employment of the 'Curvenmesser' which makes possible a more accurate determination of the actual extent and rate of the writing-process.

Diehl's conclusions are briefly as follows:

1. The extent of the stroke decreases with the difficulty of the task imposed when this increase in the difficulty takes any of the following forms, an acceleration in the rate, an abatement of the stimu-

lus, or requiring the subject to write the subject-matter in the reverse order. On the other hand, the extent of stroke increases with the lightening of the task.

2. The rate slows up when the subject-matter is written in the reverse order, and is increased by a volitional impulse from within or by a stimulus from without.

3. The pressure increases when by reason of the increased difficulty of the task a volitional impulse is called out. It decreases with the ease of the task imposed.

4. The pauses in the writing are more influenced by the changes in the difficulty of the task than are the writing-periods.

5. The writing-periods for individual figures depend primarily on the extent of stroke, and therefore on the frequency and irregularity of the changes connected therewith. The position of the figures in the series is a determining influence.

6. Practice seems to reduce the size of the characters in writing without accelerating the speed; there is a decrease of pressure with practice also.

7. Under unfavorable emotional conditions there is a retardation of speed with an increase of pressure and a reduction in size of the characters.

8. Length of stroke, writing-time and writing-pressure are characteristics which vary together from one person to another. This does not hold for the rate.

9. The writing processes of different persons are comparable, thus, only when under exactly similar conditions.

10. Women write a larger hand and more rapidly and with less pressure, that is, more easily, than men, a conclusion exactly the opposite of that of Gross.

11. Men respond to an increased difficulty in the task imposed by an intensifying of the volitional impulse, the women by a reduction in the size of the characters written.

If this research has done nothing more than to show, as it does clearly, how fundamental the problems of rhythm and the complex inter-relationships of organic habits are to the study of the phenomena of such a process as writing, it would have been well worth the painstaking work which has been put upon it. As the author says, this is only a beginning. But it is a beginning in the right direction and promises much.

H. Heath Bawden.

NEUROLOGICAL.

Recherches sur la biologie de la cellule nerveuse. By G. Marinesco. Archiv für Physiologie, 1899, Nos. 1 and 2. Pp. 89–111.

Observations, based on experiments, on the 'chromatophil' of the protoplasm of nerve cells, many valuable bibliographical references to the work of other neurologists, and considerations concerning the trophic relations of the neurones, form the chief substance of this paper by one of the numerous Italian investigators. A double-page plate made up of seven drawings of cell-structure adds something to the interest of the article.

To Professor Marinesco the nerve cell, whatever its morphology, seems to be composed in general of three essential elements: first, the chromatic substance called chromophil or chromatophil (from the ease with which it is stained); secondly, a 'figured' achromatic element (these two giving each cell its peculiar shape and character to the eye); thirdly, an amorphous substance, also achromatic, the fundamental protoplasm. The variable disposition and combinations of these three afford the neurones their very diverse appearance at different times.

The summary is of more interest to the psychologist perhaps than would be a detailed account of the often interesting observations and experiments. The part concerning the chromatophil is technically descriptive. Concerning cellular trophism so far as pertains to the nervous system, the author concludes as follows—notions which if substantiated would seem to bear not a little on the disputed continuity or discontinuity of the structural conducting units throughout the organism: "1. There is an intimate solidarity between the different constituent parts of the neurone. By interrelations, every portion plays a trophic rôle towards every other. When the protoplasmic prolongations or the prolongations of the axis cylinders are destroyed or injured in any way, the cell-body is the seat of constant reactionary changes which were unknown until within the last few years. Waller's theory therefore is obviously incomplete and inexact, inasmuch as it concentrates in the body of the cell all the trophic activities of the nerve. 2. There is a solidarity between the different neurones. Every disturbance in the function of a neurone affects the action of the adjacent neurone. For example, lesion of the sensory proneurone, following neural section or amputation, causes after a time trophic changes in the second neurone or in an indirect sensory neurone. To primary neural degen-

eration or atrophy there succeeds secondary nerve-atrophy. The process may go still further and attack the third neurone: there is then tertiary neuronal atrophy. The lesions of the cortical substance give rise to a like series of neural atrophies, but in an inverse way. 3. These facts cannot be understood if we admit, according to the classic theory, that the trophic influence is produced spontaneously in some way within the nerve-cell. For this notion of trophic automatism of the neurone, we substitute a theory which subordinates the neurone's life to the afferent (cellulipetal) and efferent (cellulifugal) excitations which pass from one neurone to the next. The functional and structural integrity of the neurone depends then at any time on the integrity of all its constituent parts and on that of the neurones which bring to it its functional stimulations. The neurone lives by its activity."

Observations on the Degeneration and Regeneration of Motor and Sensory Nerve Endings in Voluntary Muscles. G. Carl Huber. American Journal of Physiology, Vol. III., No. VII., 1 Feb., 1900. Pp. 339–344.

In this not very novel research the posterior tibial nerve in each of fifty-seven rabbits was crushed, and observations made in each case afterwards to determine the progress of degeneration distal to the injury and later on in other rabbits as to the regenerative process. This nerve is peculiarly useful for this purpose because it connects with endorgans of three sorts in the muscles: neuro-muscular, neuro-tendinous, and sensory. Periods of from twenty-three hours to six days were allowed to elapse before microscopic examination was made in search of degenerative changes, and periods of from twenty-eight days to one hundred and seventy-eight days in the case of regeneration, the tissues being in both cases stained while alive, generally with methylene-blue. Functional differences and visible changes in the endorgans seemed to correspond regularly.

We may best perhaps quote the author's own "Conclusions: 1. The motor and sensory nerve endings of voluntary muscle degenerate after severance of the muscle nerves. The motor nerve endings and the extreme distal portion of the motor nerves degenerate earlier than do the sensory nerve endings. 2. The motor and sensory nerve endings in voluntary muscle may, under suitable conditions, regenerate completely. The motor nerve endings regenerate more quickly than the sensory nerve endings. 3. These experiments, it seems to me (although this is not emphasized in this report), show clearly that the regeneration of a degenerated portion of a peripheral nerve and its

termination is brought about by the down growth of the axis cylinder of the central, undegenerated portion of the nerve fibre. Further observations in this field are in progress."

GEORGE V. N. DEARBORN.

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COMPARATIVE PSYCHOLOGY.

Notes on the Psychic Development of the Young White Rat. WILLARD S. SMALL. American Journal of Psychology, Vol. XI., No. 1, Oct., 1899. Pp. 80–100.

An Experimental Study of the Mental Processes of the Rat. WILLARD S. SMALL. American Journal of Psychology, Vol. XI., No. 2, Jan., 1900. Pp. 133–165.

Notes on the Individual Psychophysiology of the Crayfish. G. V. N. DEARBORN. American Journal of Physiology, Vol. III., No. IX., 1900.

Studies on Reactions to Stimuli in Unicellular Organisms. VI. On the Reactions of Chilomonas to Organic Acids. H. S. JENNINGS. American Journal of Physiology, Vol. III., No. IX., 1900.

The first of Mr. Small's articles describes daily observations on white rats from birth to the age of twenty-eight days. Their responses to various physical stimuli were noted and conclusions drawn concerning the growth of their sense-powers, the presence of instincts and emotional reactions and of 'intelligence.' Such records are of great value in as much as an animal's sense-powers and unlearned responses are the basis of its mental capacities and growth. It is, however, a question whether the collection of miscellaneous observations cannot well be supplemented by more definite and rigorous, even if narrower, inquiries. When, for example, Mr. Small concludes that all tastes are 'unpleasant' to the newborn rat because it squeaks and wipes with forepaws at the sugar-solution, warm milk, salt solution and clear water applied to its lips, one wishes that he had made more definite tests. It may not have been the taste at all to which the animal reacted.

Mr. Small's comments on his observations retain something of the vague phraseology and anthropomorphic descriptions of the popular writers on 'intelligence.' Surely one can find more scientific descriptive language than the following: 'One thus insulted,' 'Saw one seeking purposively,' 'declined to eat sealing-wax,' 'advancing intelligence

was marked to-day by a rat's pushing a comrade away from the dish of milk,' 'as if affectionately fondling each other,' 'they are veritably lieblichste Thierchen,' 'I think there is a considerable fund of altruism in the rat nature.'

Mr. Small tested the mental processes of adult and young rats by observing their methods of learning to overcome some simple obstacle which withheld them from food. Digging at a certain place and removing strips of paper which held a door, were the acts concerned. The field of experimentation was thus small, but the animals' behavior is recorded in minute detail and a rather elaborate analysis of the process is attempted. No signs of inference or of imitation in the sense of learning to do a thing from witnessing another do it, were seen. Mr. Small writes as if he thought that the animals concerned had habitually 'ideas' of being in and getting into the box, of eating, etc., but he offers no evidence to refute the evidence on the opposite side which I presented in 'Animal Intelligence; an Experimental Study.'

The patience shown by Mr. Small in making such detailed records is admirable, though it must be confessed that it takes some patience to read them. This article like the previous one suffers from a number of anthropomorphisms which I take it are due to carelessness of rhetoric rather than to any real misconception of animal mentality.

Dr. Dearborn describes a large number of observations on the sense-powers and behavior of the crayfish. His aim was to discover the range of variation in a number of functions among individuals and in the same individual at different times. Constancy in the performances of an individual was taken to be evidence of 'individuality.'

The range of variation in both respects was found to be very large, and no definite correlations (*e. g.*, between slowness in one test and slowness in another) could be made out. It is possible that this variation in results may have been due in part to variations in the stimulus or situation to which the animal responded. It is a very delicate task to keep the conditions of a psychological experiment constant when your subject is an animal which is sensitive to stimuli which human beings in the same situation would not notice. In experiments on traction strength, for instance, "the animal was set upon the wet towel and gently tapped with a glass rod." Here the variation in the force exerted by a crayfish at different times may have been partly due to variations in the force and place of the tap. Again we do not know that the animal in every case stopped walking ahead because it could pull the spring no farther.

It should be noted that there is a good deal of constancy in these

animals' behavior, after all; *e. g.*, when tapped, they *invariably* walked forward, though they varied exceedingly in the force they executed. The most important case of variation found by Dr. Dearborn concerns the simple ability of a crayfish to right itself when put on its back on a smooth plane surface in water. Some did so easily; some with difficulty; some never could.

In a brief paper Dr. Jennings seeks to explain the phenomena of aggregation which Garrey demonstrated in the case of Chilomonas, as results of the motor reflex described in Jennings' earlier papers. It is unfortunate that Garrey has not made use of Jennings' method and so gives no opportunity for an exact comparison of observations. If Jennings' account of the motor reaction of Chilomonas is correct, it certainly seems sufficient to explain the phenomena noted by Garrey. It is to be hoped that the significance of Jennings' studies will bring about their repetition by other observers.

EDWARD THORNDIKE.

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NEW BOOKS.

Völkerpsychologie; eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. WILHELM WUNDT. I. *Die Sprache*, Leipzig, Engelmann. 1900.

Das soziale und sittliche Leben erklärt durch die seelische Entwicklung. J. MARK BALDWIN. Translated from the second English edition by DR. R. RUEDEMANN, with preface by DR. PAUL BARTH. Leipzig, J. A. Barth, 1900. Pp. xv+466.

Annales de l'Institut international de sociologie, VI. R. Worms. 1900.

Introduction a la vie de l'esprit. L. BRUNSCHWIEG. Paris, Alcan. 1900. Pp. 176.

Medicine and the Mind. M. DE FLEURY. Trans. by S. B. COLLINS. London, Downey & Co.; New York, imported by Scribners. 1900. Pp. xii + 373.

Outlines of the Comparative Physiology and Morphology of Animals. JOSEPH LECONTE. New York, D. Appleton & Co. 1900. Pp. xviii + 499.

Dreams of a Spirit-Seer, illustrated by Dreams of Metaphysics. IMMANUEL KANT. Translated by EMANUEL F. GOERWITZ, edited with an introduction and notes by FRANK SEWELL. London, Swan, Sonnenschein & Co., Lim.; New York, The Macmillan Company. 1900. Pp. xiv + 161.

Immanuel Kant's Kritik der reinen Vernunft. B. ERDMANN. Berlin, Reimer. 1900. (5th edition.) Pp. 609.

The White Robe of Churches of the XIth Century. H. D. M. SPENCE. London, J. M. Dent & Co., imported by Charles Scribner's Sons. 1900. Pp. xx + 348. $3.

NOTES.

IT is proposed to celebrate the 70th birthday of Professor Wilhelm Wundt, which will occur on the 16th of August, 1902, by the publication of a *Festschrift* to which his former students are invited to contribute. The manuscripts must be forwarded to Professor Külpe, Würzburg, not later than January 1, 1902.

ABERDEEN UNIVERSITY has conferred the degree of Doctor of Laws on Professor Josiah Royce, of Harvard, who recently completed the second series of Gifford Lectures before the University.

PROFESSOR J. MARK BALDWIN has been given the degree of D.Sc., by Oxford University, this being the first occasion on which this degree has been conferred by Oxford.

PROFESSOR E. B. DELABARRE is this summer conducting an expedition to Labrador.

THE death is announced at the age of 87 years of M. Ravaisson Mollien, formerly professor of philosophy at Rennes, inspector general in the Department of Higher Education and curator in the Department of Antiquities at the Louvre. He was the author of many works on philosophy and æsthetics. The death is also announced of M. Hippolyte Stupuy, at the age of seventy years. He was curator of the artistic collections of the City of Paris, and the author of works on philosophy and of a biography of the mathematician, Sophie Germain.

BY the will of the late Jonas G. Clark, Worcester, Mass., who founded Clark University in 1889, the entire estate is left to the uni-

versity, providing the people of Worcester raise a fund of $500,000. If the sum of $250,000 is raised, he bequeaths $500,000. If $500,000 is raised, he bequeaths $1,000,000 and makes the university his residuary legatee. He also leaves $100,000 for the university library and $100,000 for a department of art.

THE announcement of the Department of Philosophy of Harvard University contains some additional courses and changes. Dr. R. B. Perry has been appointed Austin teaching fellow, and will give the course on the philosophy of Kant which has been given by Professor Palmer. Dr. Miller will give the second half of the introductory course in philosophy in the place of Professor Royce. Professor Santayana offers a course in Aristotle's Metaphysics.

THE following promotions have been made in the Philosophical Department of the University of Michigan: George Rebec, Ph.D. (Michigan), instructor in philosophy, to be assistant professor of philosophy; W. B. Pillsbury, Ph.D. (Cornell), instructor in psychology, to be assistant professor of psychology and director of the psychological laboratory.

DR. ARTHUR H. PIERCE, of Amherst College, has been appointed associate professor of mental and moral science at Smith College.

DR. MAX MEYER, formerly assistant in the psychological laboratory at Berlin and this year honorary fellow in psychology in Clark University, has been appointed professor of psychology in the University of Missouri.

HERBERT G. LORD, A.M. (Amherst), has been appointed professor of philosophy in Columbia University. He will have charge of the introductory collegiate courses.

DR. C. M. BAKEWELL, of Bryn Mawr College, has been called to a professorship of philosophy in the University of California and will be succeeded at Bryn Mawr by Dr. David Irons, of Cornell University, who will have the title of 'Associate.'

MISS MARGARET F. WASHBURN, Ph.D., professor of philosophy in Wells College, has been appointed Warden of Sage College, Cornell University.

MR. J. FRANK MESSENGER, B.A. (Kansas, 1895), has been appointed assistant in the psychological laboratory, Harvard University.

DR. ARTHUR WRESCHNER has qualified as docent for philosophy and psychology at Zurich. The subject of his inaugural address was 'The Influence of Leibnitz on pre-Kantian Psychology and Æsthetics.'

VOL. VII. No. 5. SEPTEMBER, 1900.

THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

I. A NEW DETERMINATION OF THE MINIMUM VISIBLE AND ITS BEARING ON LOCALIZATION AND BINOCULAR DEPTH.

BY PROFESSOR GEORGE M. STRATTON.

The smallest lateral difference of place that is visible has until recently been given as about 50″–60″ angular measure. The method employed by Helmholtz and others[1] in reaching this result was the well-known one of bringing two parallel lines together until they finally are just distinguished as *two*— on the same general principle by which Weber determined the tactile space-threshold, by finding the distance between two compass-points that just cease to merge into *one*.

But by a different method it is now evident that a lateral difference[2] of place of about 7″ of arc can be directly perceived.

Instead of using lines or points side-by-side, the experiments which give this result were made with lines end-to-end, so arranged that the upper of two perpendiculars could be moved at will to the right or left while still remaining exactly parallel to the lower line, as shown in Fig. 1. The observer had simply to judge whether the upper line was continuous with the lower, or to which side it had been displaced. In the initial trials the lines were narrow slits of light surrounded by

[1] See Helmholtz: *Physiologische Optik*, 2d ed., pp. 256 et seq.

[2] In contrast with a difference in depth or a difference not of itself perceptible, but evident merely by reason of stereoscopic effect.

a black ground—each line not more than half a millimeter wide and some 10 cm. long, and observed in a half-darkened room from a distance of 18 meters. But the surprising fineness of discrimination made it impossible to work with accuracy from so short a distance; so that finally the observer was placed 120 meters away, and the lines were changed to narrow white strips, part of the time 10 mm., part of the time 8 mm., wide, each 50 cm. long, on a dull-black ground. Each line with its ground was mounted on the face of a plate of glass, the two glasses placed edge to edge in a frame; and, by sliding the upper plate upon the lower, lateral shifts were made by steps of 1 mm. At first the ordinary procedure of 'minimal changes' was adopted, but it soon became clear that suggestion was playing too large a rôle, and resort was had to a compromise between this method and that of 'right-and-wrong cases,' which might be called a method of serial groups, to be described more fully at another time. Taking as the threshold the point where 80%, or more, of correct judgments occurred, the following results were obtained:

FIG. 1.

SUBJECT A. (187 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

RIGHT.	LEFT.
2. mm.	5. mm.
4. "	6. "
3. "	5. "
Aver. 3. mm.	Aver. 5.3 mm.

General average 4.1 mm.

SUBJECT D. (277 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

RIGHT	LEFT.
4. mm.	6. mm.
3. "	5. "
2. "	5. "
4. "	5. "
Aver. 3.2 "	Aver. 5.2 "

General average 4.2 mm.

Taking 1 mm. as equivalent, approximately, to 1.7 seconds of arc, where the radius is 120 m., we get a fraction over 7 seconds as the threshold of space-distinction under these conditions.

The experiments were begun with some doubt whether there was a direct perception of a *spatial* difference here; whether, for instance, some purely intensive change—some apparent dimming or strengthening of the impression from the adjacent ends of the lines where the displacement occurred—might not serve to suggest indirectly a spatial inequality. But anyone making the observation is soon convinced that what he sees is not of this character, but that the two lines seem to form a single line no longer straight. He seems to compare the position of one of the lines with an imaginary extension of the other, and to notice that the two, in this way, do not coincide. As a check, however, the observers throughout the experiments were kept in ignorance of the actual direction of the displacements, and the threshold was not considered as reached until the direction could be told by them; so that if their judgments had been based on anything other than the perceptible position of the lines, the fact of a break or shift in general might perhaps have been inferred, from some intensive difference in the light, but it is difficult to see how, from that alone, the subjects would have been able to tell correctly to which side the line had been moved. There seems, moreover, to be nothing inherently suspicious in the striking disparity between these results and those obtained by the older method. In the threshold obtained in Helmholtz's way these finer measurements are not reached simply because the diffusion of the stimulus from the two lines side-by-side makes a fairly uniform blur on the retina between them, and within the limits of the blur two separate objects can no longer be distinguished. But when the lines are arranged as in the present experiment, any such interference by mere diffusion is greatly lessened, and the localities can be clearly and correctly distinguished.

The result thus obtained is interesting in several ways. In the first place, it probably removes the grounds for inferring, as yet, that stereoscopic depth at its minimum is a *subconscious* result of the spatial conflict of the two images. My own ex-

periments with the pseudoscope[1] had shown that an angular difference of 24 seconds between the two impressions was sufficient to give a binocular relief. Still later, Bourdon,[2] experimenting with needles at short range, found that a difference amounting to but five seconds produces a perceptible depth-effect. As long as the conscious lateral threshold was counted as above 60 seconds, one would be tempted from these results to believe either that stereoscopic depth was not due primarily to a lateral space-discrimination of the two images, or else that the plastic effect must depend on a subconscious action, since the disparities in the image were smaller than could be consciously noted. The present reduction of the conscious lateral threshold to about 7 seconds leaves but a small margin upon which to base such conclusions, particularly when one takes into account the wide contrasts in the (non-essential) conditions of the experiments. Bourdon would seem entirely justified, however, in concluding that binocular depth cannot be due to our detecting double images, if by double images we are to understand outlines that are distinguishable *side-by-side*. But the depth-effect may still be due to the presence of double images in the sense of outlines that are felt not to be coincident when positions are compared *end-to-end*. There certainly is a difficulty in that ordinary stereoscopic vision seems hardly to provide the conditions for comparing outlines in this way. It is barely possible, however, that the curious phenomenon of retinal rivalry may be useful just herein, that by the successive emerging and disappearance of parts of the outlines in the two projected fields of view something comparable to the conditions of the present experiment is brought about, and an exceedingly fine perception of lateral incongruity results.

But perhaps a more important bearing of the experiment is on the general problem of visual localization. It seems highly improbable that so minute a displacement is discerned by noting some muscular jog or unevenness in running our eyes up and down the line, when one recalls that the fovea itself is some

[1] A Mirror Pseudoscope and the Limit of Visible Depth, PSYCHOLOGICAL REVIEW, Vol. V., p. 632.

[2] L'acuité stéréoscopique, *Revue Philosophique*, January, 1900.

300 times broader than the retinal image of the space-inequality here perceptible. In view of this relatively wide expanse of the fovea, it seems highly questionable whether the eye, in running up and down such a pair of lines would regularly take one course when the lines exactly met and a perceptibly different course when one line was displaced 7″. The eye naturally moves by twitches and jerks, even when following a straight line; the breadth of the fovea is such as to permit considerable roving without 'losing' the line. So that so small a dislocation in the objective line would probably be no incentive to an exactly corresponding change in the movement of the eye. And even supposing that a dislocation of 7″ in one of the lines did regularly tend to draw the eye by so much out of its course; how should we be able accurately to interpret so slight a variation of muscular action, as clearly due to an objective spatial inequality, when much greater movements—likewise involuntary, are constantly occurring without our interpreting them as due to a spatial variation in the object we are observing?

If, on the other hand, we pass from the muscular apparatus and look to the minute elements in the retina to explain such discriminations, even these seem much too gross to account for the marvellous fineness of our judgment. Rows of cones in the mosaic of the fovea lie apart a distance corresponding to at least an angular measurement of 30″. To explain our power to detect a spatial difference one-fourth of this, it will be necessary to assume either that the rods and cones are not the ultimate spatial elements in the retina, or else that the *minimum visible* may be considerably less than the distance between the center of the adjacent sensory elements.

And, after all, it is not difficult to see how this latter might well be the case. We must, however, first give up the notion that the light, even when it falls upon a single cone, or row of cones, affects only those elements upon which it directly falls. In every case the stimulation is probably diffused in all directions; a responsive wave runs through the neighboring elements, as is evidenced, for instance, by the fact of simultaneous contrast. Now if C and C' be adjacent elements, and b the boundary between them, then it seems not improbable that if a ray

fall somewhere within the limits of C its effect upon C' will be different according as its point of incidence is nearer or farther from b. It would probably excite C' more intensely the nearer it fell to the limits of this cone; and, on the other hand, its effect upon C'', a neighbor on the opposite side of C, would correspondingly decline. In such an event there would be a difference of nervous result with every change, however small, of the point of incidence, instead of a change occurring only in case the stimulus passed to an entirely different cone. There would then be a difference of intensity of the diffusive effect upon neighboring elements, a different degree of whatever kind of reaction may be characteristic of each, and consequently a change of 'local sign' even when the difference of place of the excitation is considerably less than the diameter of a single element. The local signs in the retina alone, quite apart from the muscular mechanism, would thus form an absolutely continuous series, and would furnish the data for any degree of spatial discrimination we may discover. The retinal sign of position is thus conceived, not often the analogy of our electric signal boxes, when one unalterable mark is given (a falling numeral, for example) whenever the same terminal is affected; but we should have to symbolize it rather by some more complicated contrivance: where nothing less than several neighboring numbers dropped on each occasion, but each of these appeared with differing clearness according as its particular terminal was near or far from the immediate origin of the disturbance. The relation of the various intensities could in this case be a sign both of direction and of distance; and the exact seat of the exciting cause be determined with a degree of accuracy depending on the fineness of discrimination for intensive differences and for catching their interrelation, rather than upon the number and distance apart of the several terminals; in other words, the threshold would not depend on the purely anatomical meas-

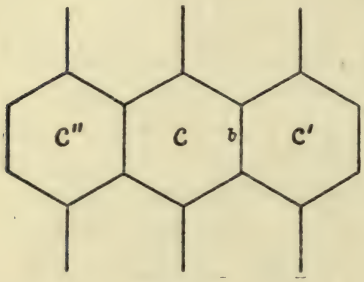

FIG. 2.

urements, if we may drop the figure and return to the retinal fact.

The present experiment, then, tempts one to believe that the local signs are of this exceedingly complicated character. The mental process of localization, or of space-distinction, cannot be justly described (it would seem) as an association merely between a particular quality of sensation and a particular place. Nor is it entirely sufficient to amend this and say that the mind must also take account of the various intensities of the quality which is spatially significant. The complication seems to go still a degree higher, so that the *interrelation* of numberless intensities of different retinal sensations would seem to be the intricate process involved in even the simplest visual perception of space. When we bear in mind that the fully organized perception without doubt includes also extra-retinal data, it is evident how complex an activity our spatial consciousness is.

II. The Effect of Imperceptible Shadows on the Judgment of Distance.

BY KNIGHT DUNLAP.

The conscious effect of stimulation of such low intensity as to be imperceptible presents an attractive and almost unexploited field for experimental work. The experiments of Jastrow and Pierce on small differences of sensation,[1] and some previous work of my own, suggested the possibility of obtaining important results from an experiment planned to show directly the effects of the presence or absence of an imperceptible stimulation.

The Müller-Lyer figure was selected as the foundation for such an experiment. If we have the segments of the principal line in the illusion-figure distinctly marked, but the angular lines of an intensity just below the threshold of perception, we have the simple materials for determining whether or not these imperceptible lines will produce in any degree the ordinary illusion-effect. If such an effect is produced, then we have

[1] Memoirs of the National Academy of Sciences, Vol. III., p. 76.

evidence for the belief that under certain conditions things of which we are not, and can not become, conscious have their immediate effects upon consciousness.

The apparatus used in the first investigations under these conditions was very simple, and not altogether satisfactory in its operation, but as the experiments were of the nature of a preliminary survey of the ground, it was not deemed advisable to make a complicated arrangement of apparatus.

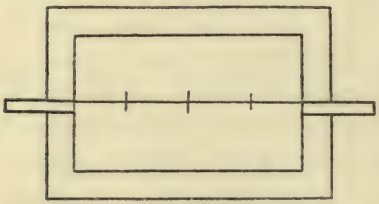

FIG. 1.

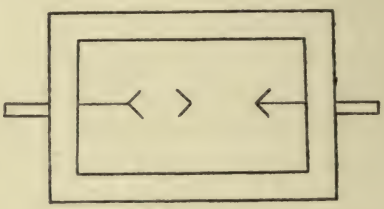

FIG. 2.

The screen upon which the illusion-figure was to be shown was a sheet of white bristol-board, fifty-eight centimeters by seventy-two centimeters, fastened upon a frame. A black line one millimeter in width (see Fig. 1) was drawn across the middle of the screen horizontally, and divided in the center by a perpendicular line three-fourths of a millimeter in width, and extending eight millimeters on each side. On the sides of the frame holding the screen, and on a level with the horizontal line were fixed guides, in which slid small steel rods passing along the line and hidden against it. Small strips of black paper two millimeters wide and ten millimeters long were fastened to the inner ends of these rods in positions parallel to the central vertical line and extending equally above and below the horizontal line, and these with the central vertical line marked off two segments of the horizontal line. The lengths of these segments could be varied at will by sliding the rods along the horizontal line.

Fastened to each of these rods carrying strips, and parallel to them, was another exactly similar rod passing behind the screen and bearing on its inner extremity an angle of ninety degrees, cut from black bristol-board, with legs five millimeters wide and

forty-five millimeters long. These angles, together with a similar one fastened in the center, showed upon the front of the screen as shadows when light was transmitted through from the back, and transformed the segments of the line into the typical Müller-Lyer figure. The arrangement of this part of the apparatus may be understoood from Figs. 1 and 2, which show respectively the front and back of the screen. It will be observed that by interchanging the pairs of rods and reversing the central angle the direction of the illusion could be reversed.

The screen was illuminated from the front by two hooded incandescent lights placed one at each side so as not to obstruct the view, and at equal distances from the screen. Behind the screen was a single hooded incandescent light, so shielded with tissue paper as to diffuse the light as evenly as possible over the back of the screen. The intensity of this light, and therefore the intensity of the shadows on the front of the screen, was controlled by means of a rheostat, the front lights remaining unchanged. As the current for the three lights was taken from the same circuit, variation in the potential affected them all in approximately the same ratio, and hence the relative intensity of the light coming through the screen to the light falling upon the face of it, as determined by the adjustment of the rheostat, might be supposed to remain fairly constant.

The method of operation was very simple. One of two figures, a double square and a circle, was placed against the back of the screen to test the intensity of the light, and the intensity of the rear light was reduced until the subject was unable to detect the shadows caused by the figure. Upon reaching a point at which it was certain that the subject could not tell whether the figure was circular or square, it was removed and the angles of the Müller-Lyer figure placed in position. It was determined by the toss of a coin whether the angles should be set in the 'short' or 'long' position,[1] and the left cross-line was fixed at twenty-five centimeters from the center. The right

[1] The direction of the angles which tends to shorten the left or standard segment of the line, thus <⟶⟶—<, will, throughout, be spoken of as 'illusion short.' The opposite direction of the angles, which tends to lengthen the left segment, will be spoken of as 'illusion long.'

cross-line was started from a point sufficiently greater or less than twenty-five centimeters from the center to be distinctly perceived as farther or nearer than the left one, and moved at regularly timed intervals[1] by steps of one millimeter towards and past the equality point, the judgment of the subject as to the length of the right segment of the line as compared with the left segment being recorded at every step in the series. Whether the cross-line should move inward from a point beyond the equality point, or outward from a point inside, was determined by lot, correction being made toward the close, however, so as to have on the whole as many series of one kind as of the other in order to offset the effect of mere direction of motion. It is evident from the details given above that the angle behind the right cross-line moved with it, so that the relations of the Müller-Lyer figure were constantly preserved.

Series of this kind alone would of course not be sufficient to determine whether or not the illusion figure is effective. As will be seen later, there is a tendency to judge the equal segments of a line as different even apart from any influence of the angles of the Müller-Lyer figure. Hence it was necessary for purposes of comparison with these to take also series in which there could be no possible illusion, since the light behind the screen was entirely cut off. If there should be any effect produced by the shadows under the conditions previously stated, a comparison of the series with those taken when the shadows of the angles were present but imperceptible might show it.

The series were consequently taken in pairs or sets, each pair being composed of one series with the shadows, and one without, taken in immediate succession, in the same direction, and from the same point. The order in which the two were taken was determined for each pair by lot, as was also the direction of the illusion as mentioned above. The subject being ignorant of the results of the lots, there was a double check upon the possibility of any influence arising from his knowledge of how the illusion might be expected to affect his judgments. Any general difference between the two classes of series could

[1] The interval was nine seconds in length from the completion of a judgment until the command to look again at the screen.

therefore only be due to the effect of the angles behind the screen.

In general, each series proceeding outward resulted in first a number of judgments of 'shorter,' then one or more of 'equal' or 'doubtful,' and finally, a number of 'longer.' In the series proceeding inward the order was reversed. The middle point of the region of doubt and equality was taken as the mean equality point of each series, and this was compared with the mean equality point of the other member of the pair. The region of doubt and equality was determined by fixed rules, and, in order to secure absolute impartiality the point was determined without the experimenter himself knowing to which class the series belonged. For this purpose a number of the records were allowed to accumulate, were shuffled, and their distinctive marks concealed until their mean equality points were determined and recorded.

Three subjects were employed, and the results of the work with them are summed up in Table I. A set was counted 'for' or 'against' the illusion according as the difference between the mean equality points of the two series composing it was or was not in the direction which would correspond to the possible effect of the illusion-figure.

TABLE I.

Subject.	Total Pairs.	For Illusion.	Against.	Neutral.
A.	23	14	8	1
R.	11	9	2	0
S.	13	8	5	0

We see from this table that 60 per cent. of the sets for subject A. fall on the side of the illusion, 81.8 per cent. for R., and 61.5 per cent. for S. The figures are rather meager, but as far as they go are strongly suggestive. As the experiments were designed only as preparatory to the investigation proper, to point to possible results and expose the difficulties in the way, the results were counted sufficient, and preparations were made for more careful experiments along the same line.

The apparatus used in the later work differed materially from that which was described above in the account of the pre-

liminary investigation. It was desired that there should be some means of measuring the relative intensities of the shadows, and as this was practically impossible when they were cast by light transmitted through the screen, it was decided to try the effect of casting the shadows directly upon the front of the screen. This of course necessitated the removal of the angles used in casting the shadows to some distance from the screen, in order that they might not be in the subject's line of vision, and this in turn demanded the use of light radiating from a very small area, that the shadows might be sharply defined. The best form of illumination available for this purpose was the electric arc, and as it is not possible to maintain the intensities of two arc lights at anything like a constant ratio, it was necessary to cast the shadows from the same source of light which furnished the general illumination of the screen. This was effected by the aid of mirrors, which diverted in opposite directions the light coming from the lamp, and combined it again upon the screen. An automatic adjustment lamp was first tried;

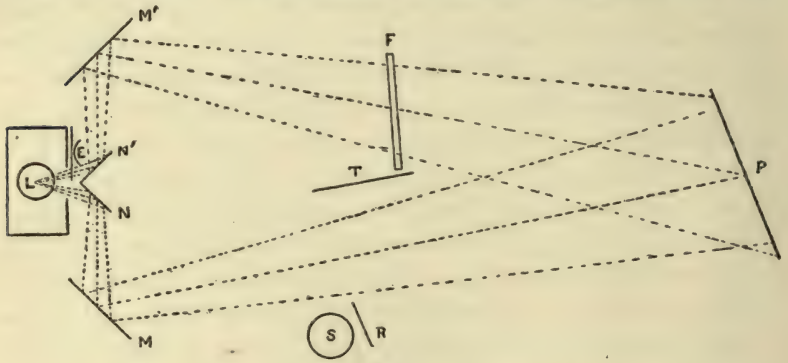

FIG. 3.

but as it proved too unsteady, a hand adjustment lamp, with carbons at right angles to each other, was constructed in the laboratory and found more satisfactory. The arrangement of the lamp with the other parts of the apparatus is shown by the diagram in Fig. 3. The lamp was placed in a box having a blackened interior and suitable apertures for the emission of light from the front, and for ventilation. The light reflected by mirrors *N* and *M* fell upon the screen *P* without interven-

tion, but that reflected by N' and M' was intercepted by the episkotister E, by which any desired proportion of the light was cut out. This light passed through the frame F, in which were suspended on silk fibers angles measuring sixty degrees (these being more favorable to the illusion effect than those of ninety degrees used in the preliminary work), and cast their shadows on the screen P. This screen was the one used in the preliminary work, with the exception of the angles at the back, which were superfluous in the present arrangement, and were therefore removed. It was placed in a position perpendicular to the line of vision of the subject at S, who was seated at one side, out of the path of the light, and hence the distribution of the light on the face of the screen was slightly unsymmetrical. This was unavoidable, however, on account of the dimensions of the room which was selected as best adapted to the purposes of the experiment. The walls, floor and ceiling of the room were black, preventing any great reflection of light back to the screen.

Some difficulty was experienced with the mirrors, and those of carefully selected plate-glass used in the experiment were not thoroughly satisfactory. Better results could probably have been obtained by the use of lenses and totally reflecting prisms.

The fibers by which the angle casting the right-hand shadow was suspended were attached to slides working in the frame F, so that the angle could be moved along as the right-hand cross-line was moved on the screen, thus keeping the vertex of the shadow approximately on the cross-line on the screen P. The principal remaining pieces of apparatus were screens, one at R to prevent the subject seeing the figure at P except at the proper time; and one at T to conceal the angles in the frame F at all times. The distance from the subject to the figure was about two and a quarter meters.

It will be observed that since the two pencils of light were taken from practically the same side of the glowing carbon, the effective area of which was only a few millimeters in extent, variations in the intensity of the light affected them both approximately equally as regards their initial intensities, and hence did not change the relative intensities as established by the episkotister. Since therefore the intensity of the light did

not vary greatly, the perceptibility of the shadows should have remained constant according to the general statement of Weber's law.

What has been said concerning methods in the preliminary work will, with some important exceptions, apply to the main work also. The proper intensity of the shadows was determined for each subject by careful tests, and these were frequently repeated during the course of the experiment to insure the correctness of the intensity adopted. It was found that subjects A., R. and W. could not detect any shadows when the total angular opening in the episkotister was seven degrees. For the sake of safety six degrees was used in the actual work with these subjects. S. at one time seemed to perceive the shadows with an aperture of seven degrees, and therefore five degrees was fixed upon as entirely safe for him. These proportions are rather large as compared with the figures usually given for the difference-threshold of light, but it should be remembered that the shadows were not perfectly distinct, on account of the effect of the fringe of luminosity surrounding the effective portion of the positive carbon, and also on account of multiple reflection in the mirrors.

As it took some time to change the direction of the angles in the frame, they were placed at the beginning of each experiment-period in the direction determined by lot, and continued in the same direction during the whole period (three-quarters of an hour), unless (as happened some few times) the subject accidentally became aware of the direction of the angles. In such a case a coin was tossed to determine whether or not the angles should be reversed, the subject being ignorant of the outcome of the lot, and so not knowing whether the shadows were continued in the position in which he saw them, or were reversed, or were entirely removed and a shadowless series commenced. Towards the end of the work the angles were arbitrarily placed so as to have an equal number of series for each of the two directions of the illusion. To economize time, the angle which cast the right shadow was not moved each time the corresponding cross-line was moved, but only every fifth time, the width of the shadow allowing that amount of movement without complete disconnection.

No attempt was made to run series in both directions, as it was not indispensable for the comparison of the series, and there were enough complications without this additional factor. All the series were run outward from a point nearer to the center than the standard length of twenty-five centimeters.

When the shadows were removed, in the shadowless series, by cutting off the light normally reflected by mirrors M' and N', the episkotister was kept running, and the angle was moved in the frame at the usual times in these series as in the others, so that the subject had no clue as to whether or not the shadows were present in any series.

For fear lest the imperfections in the mirrors might produce irregular distribution of the light on the screen in such a way as to affect the results of the series, the two mirrors on one side were interchanged from time to time with those on the other side, and an equal number of series taken in each of the two positions. There were thus three conditions to be equalized, viz: (1) Position of the mirrors; (2) Direction of the illusion; (3) Order of series in the set, *i. e.*, whether the series with the shadows was given first, immediately succeeded by the series without the shadows, or *vice versa;* in the scheme which follows the first class are called 'shadows first' and the second 'shadows last.'

This classification broke the sets of series up into eight groups, which were kept equal in the long run (with one exception, to be mentioned later), although the sets into which the series fell were determined by lot from day to day as far as possible. These groups are given schematically in the following table:

1st position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.
2d position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.

On account of the complication of the apparatus and the difficulties of its operation, the progress of the experiment was necessarily slow. No attempt was made to have the subject judge at regularly timed intervals. An endeavor was made, however, to give all series with the same approximate rapidity, and the rate of ten minutes to a set was pretty constantly maintained. This allowed the taking of three sets or pairs in a period of forty-five minutes, if all worked smoothly, the rest of the time being consumed in the necessary adjustments of the apparatus. Very frequently, however, troublesome delays occurred, which reduced the number of sets to two or one.

Each subject was instructed to preserve a fixed method of bringing his eyes upon the line. One preferred to bring his eyes upward from the floor to the middle of the line after removing the swinging screen, while the others preferred to gaze at the swinging screen, in the direction of the center of the line, and then displace it. This regularity was insisted upon because it was found by actual trial that the direction in which the eyes were brought upon the screen influenced the proportional estimate of the segments of the line. Therefore a difference in this respect between the different series might introduce a difference in the results which would confuse the interpretation.

The subject was not however compelled to maintain his gaze fixed upon the central point, but moved his eyes over the line as he pleased in forming his judgment. In the extreme cases the disparity of the segments of the line was perceived by glancing at the center, but where the difference was very slight the subject gazed at one segment until he had its length well in mind and then transferred his regard to the other segment in order to make comparison.

It is probable, however, that the steady fixation of the segments was on the whole a bad practice, as it rendered possible the formation of an after-image of the first segment by which the second segment might be judged by mere superposition, which would be in a large measure destructive of the effect of the illusion, if there were any such effect. The difficulties of judging were however so great that it was not deemed advisable to place any additional restrictions on the subject.

The experiment would naturally have been much simplified if the shadowless series had been omitted, and the sets of two made to consist of one with the illusion short and one with it long. This would have reduced the number of groups to four, and would perhaps have nearly doubled the difference between the series on each set, but it was deemed inadvisable for several reasons. First, the changing of the direction of the angles for each series would have consumed so much time that the gain by omitting the shadowless series would have been largely offset. Second, the data for the judgments of the segments of the line with the influence of the illusion excluded are important in themselves. And third, it was considered possible that under the conditions, the illusion, if effective at all, might be more effective in one position or direction than in the other; a state of affairs of which the proposed method would offer no evidence, should it actually exist. The results as set forth later justify the procedure actually adopted, although additional experiments ought to be made in the proposed manner to supplement the results of these.

One more point as to the methods, and then we may pass on to the next division of the subject. The series were always commenced far enough inside of the actual equality-point to give a distinct impression of shortness to the right segment of the line. They were not however commenced uniformly at the same point, but were varied irregularly in this respect from set to set, and the subject was given to understand that they were irregular, lest the distribution of his judgments in one series should influence him in the next. Both of the two series of each set were of course commenced at the same point, so that any possible effect of the length of the series, or the strength of the contrast with which the series began, might affect them both alike; but this was not mentioned to the subject, who consequently never knew whether a given series began at the same point as the preceding. It is interesting to note that the general region of commencing the series was necessarily varied from day to day and even from set to set on account of the varying relative estimation of the segments of the line. Thus a point which one day was well within the range of judgments

of 'short' might be the next day within the range of judgments of 'equal.' This will be further exemplified later in the discussion.

Four subjects were employed, three of whom, S., R. and A., had taken part in the preliminary work. In the cases of S. and R. eighty series (forty sets) were taken from each, evenly distributed according to the scheme of the eight classes of sets already specified; but in the case of A. only sixty-four series (thirty-two sets) evenly distributed, and in the case of the other subject, W., seventy-two series (thirty-six sets, twenty under the class 'long' and sixteen under the class 'short') were obtained.

The most obvious method of comparison of the series was by sets, taken in the same manner as that in which the series in the preliminary work were compared. The same precautions for impartiality in computation were observed here also. The results of this comparison as given in Table II. are far more striking than the results for the preliminary work.

TABLE II.

Subject.	Total Sets.	For.	Against.	Neutral.
S.	40	23	15	2
W.	36	19	11	6
R.	40	25	9	6
A.	32	22	7	3

Expressed in percentages this table becomes Table III., as follows:

TABLE III.

Subject.	% for.	% against.	% neutral.
S.	57.5	37.5	5.0
W.	52.8	30.5	16.7
R.	62.5	22.5	15.0
A.	68.8	21.9	9.3

This shows a preponderance for the illusion; twenty per cent. for S., over thirty-two per cent. for W., forty per cent. for R. and over forty-six per cent. for A. Or, if we leave out of account the neutral cases and give directly the ratio of favorable to unfavorable cases, we find the following values: S., 1.5.

W., 1.7; R., 2.8, and A., 3.1, which are certainly too large and agree too well to be set down as the result of mere chance.

A second comparison naturally suggested would be between the average of the mean equality-points for the shadowless series and the averages for the series with the illusion-figure in both positions. An examination of the series, however, showed that the average for all of the shadowless series could not be fairly compared with the averages for each of the other two classes, since the series for the two latter occurred in groups irregularly distributed throughout the time the experiment was continued, and the general position of the mean equality-point varied greatly during the progress of the work.

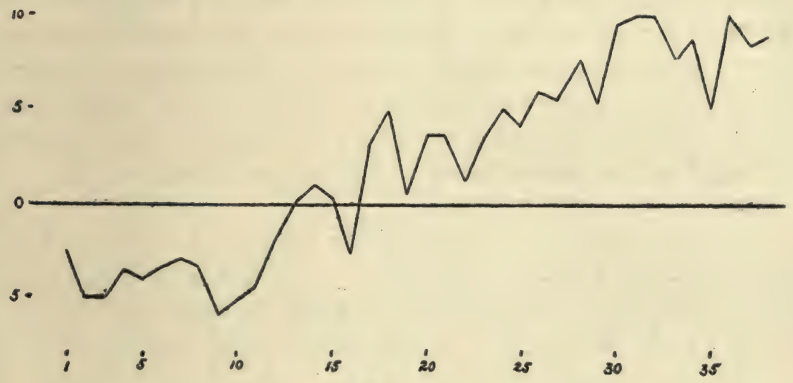

FIG. 4.

This is illustrated by Fig. 4, which gives the position of the mean apparent equality-points for each of the shadowless series of experiments with subject W. In this diagram the values of the abscissa represent the order of the series in the course of the experiment, while the ordinates give the difference in millimeters between the standard line and that length of the variable which seemed equal to it. Positive ordinates, therefore, show that the apparently equal variable was actually longer than the standard, while negative ordinates indicate that it was shorter.

All of the graphs thus obtained were very irregular, and those for subjects W., R. and A. show a decided upward tendency from first to last, indicating that while early in the experiment the right segment was proportionately overestimated, later it

was underestimated. Subject S., with a single exception, over estimated the right segment throughout the experiment, rather more toward the last than the first.

The graphs must not be understood as analogous to the filling in of the points of a probable curve between known points, as in this case the points represent discrete series and there are no possible points which might be computed between them. Only the known points are significant therefore, the lines joining them having been added to distinguish the trend of the series and make it more easily comprehended by the eye.

The ordinates for the series with shadows would correspond pretty closely with the ordinates for the shadowless series with which they belong; but, as previously noted, the two classes of series with shadows were not distributed regularly over the interval covered by the shadowless series. In the case of one of the subjects, for example, the fall of the lots was such that most of the series with illusion 'long' occurred at the upper position of the graph for the shadowless series, and those with illusion 'short' at the lower positions.

It would, therefore, be manifestly incorrect to compare series corresponding to the upper part of the graph with series corresponding to both upper and lower parts, as this would bring in a difference due merely to the progress of the experiment.

TABLE IV.

AVERAGE MEAN EQUALITY POINTS.

Subject.	Illusion Long.		Diff.	Illusion Short.		Diff.
	Shadowless.	Shadow.		Shadowless.	Shadow.	
S.	—6.42	—5.8	+.62	—5.57	—5.75	— .18
W.	+2.37	+2.77	+.40	+2.65	+1.75	— .10
R.	—3.57	—3.40	+.17	—2.40	—3.45	—1.05
A.	—0.37	+0.46	+.83	+1.06	+0.78	— .72

Hence in computing the averages, those for the shadowless series which were taken at the same time as the series with the illusion 'long' were kept separate from the averages for the shadowless series taken at the same time as the series in which the illusion was 'short.' In other words, the averages for the two classes of pairs, as distinguished by the direction of the

illusion, were taken independently. The results for this operation are given in Table IV.

The figures in the first, second, fourth and fifth columns of this table represent the distance in millimeters from the actual

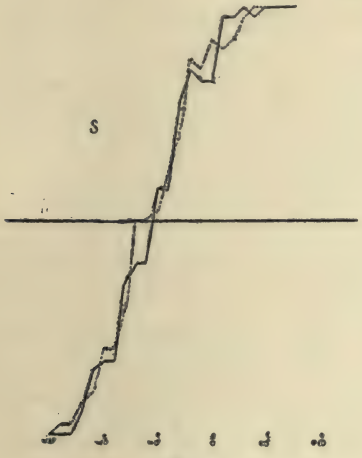

FIG. 5.

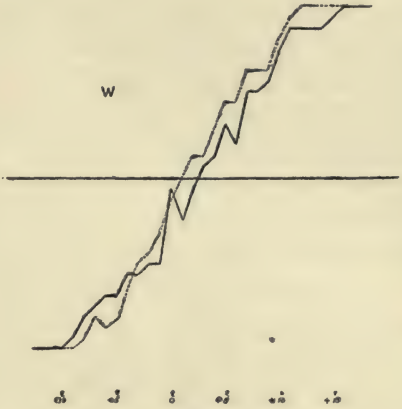

FIG. 6.

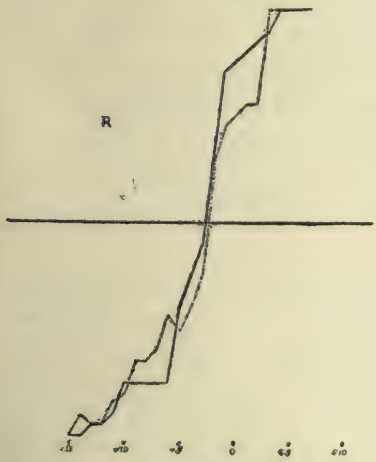

FIG. 7.

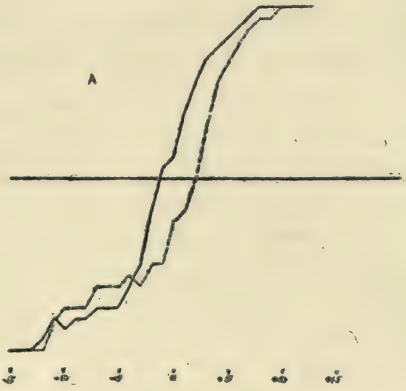

FIG. 8.

equality point to the average mean equality point for each group of series as indicated by the column headings. Negative distances are measured from the actual equality point towards the center of the line in the illusion-figure, and positive distances in

the opposite direction. The figures in the third and sixth columns give the differences of the averages, and hence directly the average change in length of the right segment when the imperceptible shadows were present.

It will be seen from Table IV. that in every case the average mean equality point for the series with shadows is farther out than that for the shadowless series when the illusion is 'long,' and just the reverse when the illusion is 'short'; which is exactly what might be expected if the illusion were really operative. This unanimity is certainly strongly in favor of the theory that the imperceptible shadows actually affect the judgment. The difference here, as in the other comparisons, is slight, but we should hardly expect to get more than a slight effect from the shadows under the circumstances.

This method of comparison by means of equality points necessarily leaves out of account certain scattering judgments which are bound to occur from time to time, and which are ignored by the fixed rules under which the mean equality point was determined in each series. We must, therefore, have some other method of comparison which shall take these into account, although their importance is not so great as is that of the more regularly occurring judgments. The sporadic judgment, contradicted by those which immediately precede and follow it, is of course largely due to a sudden subjective change in the observer, but its significance lies in the fact that the objective conditions might allow these subjective conditions to be more effective in one case than in another.

The method adopted for doing justice to these scattering judgments was the comparison of the total number of judgments of each kind (*i. e.*, 'longer,' 'shorter,' and 'equal' or 'doubtful') for each value of the variable lines on the screen, for each of the two classes of series with the illusion shadows, with the corresponding classes without the shadows. Thus, for example, the totals of the different kinds of judgments when the variable was 23.5 cm. (the standard being 25 cm.), when the illusion was 'long,' were compared with the totals for the same point in the corresponding shadowless series; and so for all of the other lengths of the variable which were used in the experiment.

Here as before, the precaution was taken to compare each of the two classes of series in which the shadows were present only with the shadowless series taken in the same sets.

There being three kinds of judgments, viz., 'longer,'

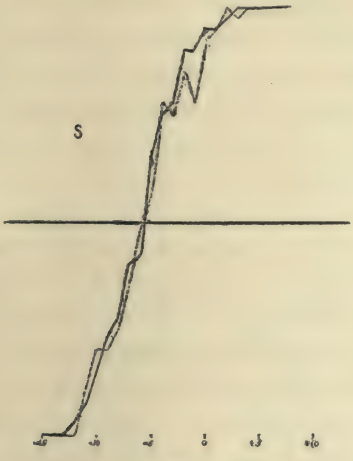

FIG. 9.

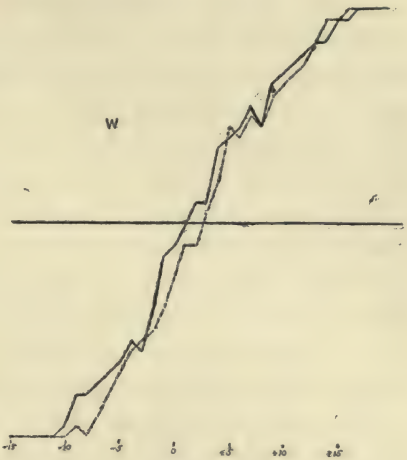

FIG. 10.

FIG. 11.

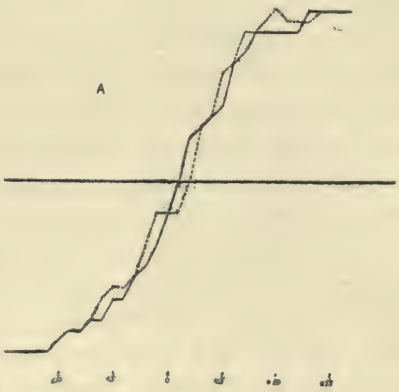

FIG. 12.

'shorter' and 'equal' or 'doubtful,' there were then for each class three totals for each point in the scale. As each of these totals is significant only in so far as account is taken of the distribution of the remaining judgments between the two remain-

ing totals, no direct comparison of the total judgments 'longer,' *e. g.*, in the shadowless series, with similar judgments in the opposite series could be made with advantage. Accordingly the judgments of 'short' were taken as negative, the judgments of 'long' as positive, and the judgments of 'equal' or 'doubtful' as zero. The algebraic sum of these judgments then, at a single point on the scale when the shadows were present, or, on the other hand, when they were absent, fairly represents the character of the total of the judgments at that point. As the series were always begun well within the region of apparent 'shortness' and ended well outside of the region of equality, it was assumed that it was possible to supply judgments at either end of those series that did not reach the limits of the extent of the scale taken for comparison, thus keeping equal the total number of judgments throughout the scale.

Figs. 5 to 12 give in a graphic form the results of this comparison. The abscissas represent the different lengths of the variable line expressed as distances in millimeters from the point where standard and variable were equal; negative values, of course, representing a variable shorter than the standard, and positive a longer. Hence the values here given, plus twenty-five centimeters, show the actual length of the variable. The ordinates represent the algebraic sum of positive, negative and zero judgments. The continuous line gives the value for shadowless series, the dotted line that for series with shadows—in Figs. 5 to 8 so placed as to give the illusion 'short'; in Figs. 9 to 12 'long.'

When treated in this way the results are less striking than in the case of the tables of general averages. If an effect of imperceptible shadows were to appear in the graphs, it would in Figs. 5 to 8 be evidenced by the fact that the ordinates of points on the dotted lines would incline more toward positive values than the corresponding ordinates of the continuous lines; and more toward negative values in Figs. 8 to 12. This would bring the dotted line to the left of the continuous line in the first group of figures, and to the right in the second group. In some of the actual curves, however, the dotted line shows no decided tendency either way (Figs. 5, 8 and 9). In Figs. 6, 10 and

11 the relation is faintly in accord with what we should expect if the shadows were effective; while in 7 and 12 the agreement is quite striking. If this accord were a mere matter of chance, several of the curves ought to be not simply neutral, but positively opposed to this course. None of the graphs, however, has this character. So that, on the whole, this third mode of treating the results also is favorable to the view that the shadows were operative in determining the judgment.

We may now sum up the results of our investigation. We have found three main points. First, when we compare the series set by set as they were taken, we find that for each subject the large majority of the sets compare as they should if the illusion were operative. Second, we find that the averages of the sets compare without exception as they should under the foregoing hypothesis, a result which does not necessarily follow from the foregoing result. Third, we find that the totals of the judgments at the various points on the scale are, to a remarkable extent, in conformity with the same hypothesis. These three methods of comparison cover every relation of the series which can fairly demand attention, and hence may be taken as exhaustive of the results of the present experiment.

A question of such importance as that with which the present discussion deals requires, however, the maximum of careful investigation before we dare call it settled. The results detailed above strongly suggest that the imperceptible illusion-figure is active in producing psychical results, but for the sake of conclusiveness additional experiments should be carried out so as to permit the comparison of series in which the shadows are in one position directly with other series in which the shadows are in the opposite position, using a modification of the method of right and wrong cases, consisting in plotting the error curve for the progressive intensities of the shadows by the method of Least Squares, and then comparing this with the empirical results obtained by the actual trial of the different intensities. This method of procedure is not absolutely necessary, but is advisable on account of the added satisfaction derived from approaching the same problem by diverse methods.

VISUAL PERCEPTION DURING EYE MOVEMENT.

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SECTION I. TWO SOURCES OF ERROR IN INVESTIGATING THE PROBLEM.

The problem of determining the character of visual perception during the movement of the eye from one point of fixation to another presents some exceptional difficulties to a psychological investigation.

During the visual examination of an object of any considerable size, say 5°, we are commonly conscious of a series of eye movements 'following the contour lines,' etc. On the other hand, the careful examination of a minute part of such an object certainly demands more than a continuous movement of the point of regard across it. We fixate it directly for an appreciable length of time.

If one endeavors to distinguish by mere introspection between the visual data acquired during the eye movements and those of the fixation pauses, an insurmountable difficulty will be discovered, viz: All self-observation of the eye movements and fixation pauses is utterly unreliable.

Attention has recently been called to this difficulty in a number of ways. It is now well known, for instance, that the eyes make a very regular series of full stops in reading; but even the trained psychologist is unable to give a reliable account of the points fixated at these stops, or even of their number, from mere self-observation.

A less experienced observer is quite unable to tell whether a given eye movement was broken by a stop or not; and even after more than five years' study of the eye movements, I have no confidence in my own judgment in the matter, if it is

unverified either by the direct observation of an assistant or by some form of registration.

Direct observation by an assistant is by far the simplest and least questionable control. It has been used throughout the following experiments. Only one precaution is worth mentioning; and that is so obvious from general principles that it needs no elaboration. The assistant must not attempt to follow any point of the moving eyeball in its excursions; but must rather fixate some point of the lid or eyelash, which is relatively at rest. In some cases it may even be necessary to construct an artificial fixation point, independent of the eye.

While this fundamental difficulty is so easily overcome by the simplest experimental conditions, it is obviously not impossible that, without these, it may have led even trained investigators into believing they saw during eye movement that which was really seen during an unnoticed fixation pause.

A second source of error in the investigation of our problem is the familiar difficulty of distinguishing the centrally conditioned elements of a percept from those which are peripherally conditioned. It is perhaps most widely known as the proof-reader's fallacy. A striking example is a phenomenon, well known to all who have worked with the tachistoscope. When a long word or phrase is grasped, even those parts seem to stand out clear and distinct, which a continuous exposure shows to have been so far out of the field of direct vision as to present a hopelessly indistinct blur.

We have no subjective criterion by which to distinguish the perceptive from the apperceptive elements in any given percept. Hence it follows that when we clearly recognize parts of a field of vision which a careful test shows to have been peripherally indistinct during all of the fixation pauses of the eye, the conclusion is almost inevitable that the clear perception was due to clearer vision during eye movement.

This second source of error is not as easily verified and controlled as the first; nevertheless it is the first thesis of this paper that the apparent clear perception of an object during eye movement is an *illusion*, due to these two fallacies of self-observation.

Section II. Clear Perception of a Complex Field of Vision During Eye Movement is Impossible.

The first attempt to determine how much of a complex field of vision could be apprehended during the uninterrupted movement of the eyes was made, so far as I am aware, by Professor Benno Erdmann and the author,[1] in an attempt to explain the meaning of those strangely rhythmic pauses of the eye in reading every page of printed matter.

It was then we chanced on the observation that when the head was held perfectly still we could never catch our own eyes moving in a mirror. One may watch one's eyes as closely as possible, even with the aid of a concave reflector, whether one looks from one eye to the other, or from some more distant object to one's own eyes, the eyes may be seen now in one position and now in another, but never in motion.

This seemed to me and still seems explicable in only one way.

The simple experiment is singularly precise. Since the phenomenon to be observed only occurs while the eye is moving, a pause in the movement, even if unnoticed, can do no harm. Even the apperceptive illusion has no chance, since it would tend, not to constructive imagination of a movement, but rather towards a greater clearness of the peripheral parts of the field of vision. There are two new sources of error, however, which demand experimental precautions. Slight, involuntary movements of the head produce conditions in which the eyes seem to move. They are best prevented by a Helmholtz head rest. Again, any abnormality in the movement of either eye, which causes it to move slower than the other, will make the end of its movement visible to the eye already at rest. This makes it advisable to confine the experiment to a single eye, shutting out the other by a screen.

The same lack of clear perception during eye movement may be discovered when the object is stationary, provided one takes the necessary precautions against the ever-threatening illusions.

If one fixates the beginning of a line on this page, for ex-

[1] Psychologische Untersuchungen über das Lesen, von Benno Erdmann und Raymond Dodge, page 69 et fol.

ample, the words in the middle of the line will be altogether illegible. The same will be true when the right end of the line is fixated, though the general character of the line is seen from both fixation points. If we look quickly from one end of the line to the other, it will seem as though we saw the letters and words during the movement, but an analysis of what was seen will show that we have not seen a particle more than we saw from one fixation point or the other.

This was the experiment by which Professor Erdmann and the author demonstrated the fact that the rhythmic pauses in reading are the moments of significant stimulation. Doubtless most who try it carefully will fall victims to the first difficulty. It will be clear that when the eye moves as rapidly as possible from one fixation point to the other nothing new is seen; but it will seem that, when the eye moves more slowly, the entire line is seen very distinctly. If the observer takes the precaution to have some one watch his eyes, as recommended, he will find that what in self-observation passes for a slow movement of the eyes is in reality broken by one or more clearly defined full stops. An assistant watching the observer's eyes will be able to predict with absolute correctness when the middle of the line was clearly seen, and when not, by the presence or absence of these full stops.

The rapid alternation of black and white stimulation by a line of letters is not the only condition of eye movement blindness. If a simple letter or figure is placed between two fixation points so as to be irrecognizable from both, no eye movement is found to make it clear, which does not show a full stop between them. Four or five lines, close enough together to prevent counting, may be substituted for the letter with the same result. This is more easily explicable when the lines are perpendicular to the line of movement, but the same result also obtains when they are parallel.

Any attempt to explain these phenomena by reference to the known laws of the action of a rapid succession of stimuli is met by the problem, which Professor Cattell emphasized in a recent significant article;[1] viz, why we get no characteristic lines

[1] On Relations of Time and Space in Vision, by Professor J. McKeen Cattell. PSYCHOLOGICAL REVIEW, vol. 4, 325 et fol.

of fusion. It is true that I believe I can detect such faint gray lines of fusion; for example, during long eye movements across a page of print; but the great majority of those I have tested discover nothing of the sort.

Section III. The Hypothesis of Anæsthesia During Eye Movement.

The possibility early occurred to me that the movement of the eye might condition a momentary visual anæsthesia. This seemed to be supported by the fading of after images during eye movement and similar phenomena. This hypothesis is, however, false. It is rendered improbable by the perception of long streaks of light seen when the retina moves across the image of a bright point of light on a dark ground. It is disproved by the following experiment:

A disk of black cardboard thirteen inches in diameter, in which a circle of one-eighth inch round holes, one-half inch apart, had been punched close to the periphery all around, was made to revolve at such a velocity that, while the light from the holes fused to a bright circle when the eye was at rest, when the eye moved in the direction of the disk's rotation from one fixation point, seen through the fused circle of light, to another one inch distant, three clear-cut round holes were seen much brighter than the band of light out of which they seemed to emerge. This was only possible when the velocity of the holes was sufficient to keep their images at exactly the same spot on the retina during the movement of the eye. The significant thing is that the individual round spots of light thus seen, were much more intense than the fused line of light seen while the eyes were at rest. Neither my assistant nor I was able to detect any difference in brightness between them and the background when altogether unobstructed.

In passing, it may be worth mentioning that the required result was reached when the holes had a velocity of one inch in 22σ, and the arc of eye movements was 4.7°

The lack of suitable regulation for my disk prevents my following out the experiment; but it is certainly striking that this result is much closer in accord with my measurements of

the eyes' velocity by the Lamanski method, than with the measurements obtained by the eye cup and pointer method by Huey. The experiment seems well worth while carrying further by some one who has access to suitable apparatus.

Section IV. Perception of a Simple Field of Vision Exposed Only During Eye Movement.

In all the direct experiments thus far published, except the self observation in a mirror, the influence of the vague peripheral image of the object seen during the full stops at the first and second fixation points unquestionably interferes with any attempt to determine just what is seen during the eye movement. The tendency is towards a belief that the same amount is seen during the eye movement as at the more favorable fixation point.

Adequate experimental conditions for the investigation of the positive side of our problem demand a complete separation of the fields of vision during the movement and during the periods of fixation. An exposure apparatus is required, depending for its action on the movement of the eyes, so that objects will be exposed during movement and covered when the eye is at rest.

The conditions for such an arrangement are furnished by the movement of the eye itself. Since the axis of rotation is within the eyeball, in every eye movement the pupil travels through an arc equal to the arc of movement of the point of regard. If the arc of movement is large enough, the pupil may be made to travel the entire width of an open slit in a screen, through which objects behind the screen will be exposed as the eye is passing the slit, while they will be completely hidden by the screen at both termini of the movement. Taking 24 mm. as the diameter of the eyeball and 5 mm. as the ordinary maximum diameter of the pupil in daylight, it will be seen that the arc of movement which would carry the pupil entirely across a slit 5 mm. wide would be 72°. In practice, however, it is better to have the slit somewhat smaller and the arc of movement about 80°.

The necessary apparatus may be easily built up from material in every laboratory. As used in the first part of the fol-

lowing experiments it was essentially a simple perimeter, fixed in the horizontal plane, to which was attached a firm head rest and a special eye-piece. This latter was composed of two screens of black cardboard, set at an angle of about 30°, converging immediately before the eye, and so adjusted that when the eye looked at the center of the perimeter it looked between the screens through a slit 4 mm. wide, standing about 4 mm. from the cornea. The length of the exposed surfaces may be 15° or even more. The best results were obtained when they were not longer than 10°. All reflection from the black cardboard should be carefully shut out of the eye by secondary screens. For fixation points small bright steel rods were used. They effect a minimum retinal area and are very distinct. Whatever the fixation point used, each should be visible from the other.

The first experiments with this apparatus were altogether negative. White cardboard filling the whole ten degrees of the field of exposure was wholly unseen. The result was the same when colored surfaces were substituted for the white.

Fearing the unusually long movement might be an unfavorable condition, a modification of the apparatus was devised to shorten the arc of movement to 50°. The plan of this modification was to substitute the insensibility of the blind spot for the opacity of one of the screens of the eye-piece. It was effected by reducing the field of exposure to 3.5°, removing the screen *a*, and placing the primary fixation point in such a position for each observer that the field to be exposed fell completely within the blind spot.

For convenience of comparison the two pieces of apparatus were made entirely separate; the second form being built up on an ordinary perimeter.

This apparatus has an especial experimental value besides shortening the arc of movement; viz, the exposure is made during the first part of the movement instead of in the middle. It is, however, not without its disadvantages. For some eyes the primary fixation point must be 1°–2° higher than the field of exposure, if the latter is to fall comfortably within the blind spot. This renders the observations in so far questionable as

the field of clearest vision is not stimulated by the exposure. Moreover, the movement can be made to advantage with each eye in only one direction; since it is impossible to look from the second fixation point to the first with sufficient accuracy to insure the complete disappearance of the field of exposure within the blind spot, in a single uninterrupted movement.

The results of the first experiments with the new form of apparatus were also entirely negative. Five different colors and white were exposed in the field five times each for both my assistant and myself, with the result that, when the eye movement was unbroken, the observer was unable to tell what had been exposed or even that anything at all had broken the black of the perimeter. My own primary fixation point was directly in line with the upper edge of the field of exposure. My assistant's was 1°30′ above.

Subsequent experiments under exactly the same objective conditions have given quite different results. But I believe these first experiments are important as showing the difficulty of perceiving the simplest possible change in the field of vision, during eye movement, without special training. It is certainly suggestive of a central explanation for the absence of bands of fusion under ordinary conditions.

These failures suggested an increase of the illumination of the field of exposure. A ground glass plate, illuminated from behind by a bright, sunlit landscape, was substituted for the white cardboard in the first form of apparatus. Under these conditions a long band of light was immediately evident at each movement of the eye. This band uniformly appeared longer than 10° even when the length was known. There were no sharp boundary lines; particularly was this true in the direction of movement. This is probably due to a peculiarity of the apparatus, which is of great service in experiments on fusion. Each screen bounding the open slit is really an opaque body, intervening between the eye and the object, and exposing first one part and then the rest as the edge of the pupil moves across its edge. If the slit is near the eye, however, this function is entirely overshadowed by another, viz: that of a diaphragm, increasing and decreasing the brightness of the image

on the retina. Thus, when the pupil moves into the pencil of rays which has passed the slit, a point may be found at which a very faint image of part of the 10° field of exposure can be seen before the other part comes into view. This first faint stimulation, as well as the corresponding stimulation as the pupil passes out of the pencil of light, affects a portion of the retina not further stimulated, giving rise to the very faint ends of the band of light as it passes off into black. In experiments with a complex color field these ends are too faint and too far from the field of clear vision to cause any inconvenience.

Except for the indefinite edges, the band of light appeared homogeneous, but very much less intense than the ground glass during direct fixation.

If colored gelatin plates were placed in front of the ground glass, the colors, though dull, were always distinguished from gray and from the other primary colors.

Under the influence of these experiments, a return to the previous conditions showed a decided difference in ability to see the bands of light. At best, however, they had a vague cloud-like appearance, and the colors were mere tintings of this cloud.

Section V. Perception of a Complex Field of Vision Exposed only During Eye Movement.

The experiments have thus far dealt only with the simplest possible fields of light or color; but the qualitative modification of these fields by the movement of the eye clearly indicates the influence of their background. The problem of this influence is of the highest theoretical importance. Professor Cattell holds that the differences in the field of vision do not fuse during eye movement; but that, on the contrary, its various elements may be distinguished with extraordinary ease.

It is certain that we do not ordinarily find in consciousness any sign of a fused field of vision during eye movement. The long failure of physiological optics to correct the illusion of clear vision during eye movement is undoubtedly due in large measure to this lack of a fusion consciousness. In spite of it, however, it is now possible to show experimentally that when

a complex field of vision is perceived during eye movement it is seen fused.

A part of a page of print exposed in place of the white cardboard in the original apparatus gave a perfect though shadowy series of gray bands on a lighter gray background, in which individual letters or words were absolutely irrecognizable. The gray bands seemed homogeneous, without sharp boundary lines and of uncertain length.

Two white surfaces separated by a vertical black stripe, 2° 30′ in width, gave a homogeneous gray band, again without sharp boundaries and of uncertain length. There was no trace of an independent black stripe.

The last form of apparatus described was by far the most satisfactory arrangement for the study of color fusions. In front of the ground glass plate, illuminated as before by a bright landscape, was brought a black screen, perforated by four vertical slits which could be filled with colored films at pleasure. Each slit was 1° in width and four times as long as it was wide. They were separated from each other by equally large sections of the opaque screen. In front of this color screen was placed a movable screen, with teeth rising to the middle of the slits in the first screen, so arranged as to cover at will the lower half of either set of alternate slits. Since the same color always filled the alternate slits of the color screen this gave a valuable basis for the comparison of the dull band of apparent fusion of colors, with a juxtaposed band, first of one color and then of the other, as the sliding screen was adjusted.

The resulting fusions were thoroughly normal.

Colorless bands resulted from filling the alternate slits with complementary colors. Red and yellow gave an orange band; red and blue a purple one.

Although the difference between the upper and lower half of the field, *i. e.*, between the mixed and the simple colors, was always clear, the bands had throughout the characteristics previously mentioned. They were elongated, dull in color, and had uncertain boundaries. In no case of unbroken eye movement were the contents of the different slits distinguishable.

Section VI. The General Failure to See a Fused Field of Vision During Ordinary Eye Movement.

The question naturally arises why we fail to notice the fusion which the experiments clearly show must occur with almost every movement of the eye from one point of regard to another. Their persistent absence is, I am convinced, centrally as well as peripherally conditioned. It is my opinion that the difference between the results of the first and second series of experiments, with the first apparatus, under the same objective conditions, was due chiefly to a difference of apperceptive predisposition and attention; clearly indicating a most important factor in any explanation of the non-perception of the faint stimuli during ordinary eye movement. If our attention were habitually attracted by them, it must seriously disturb our clear vision at each new fixation. We should, consequently, rather expect them, like the entoptic phenomena, to be systematically ignored and even to require experimental conditions to demonstrate their existence.

In respect to the peripheral conditions, we must remember that it is only in complex fields of vision that the problem has any meaning. If the field of vision is simple or homogeneous in the planes perpendicular to the axis of rotation of the eye, stimulation during eye movement could not be differentiated in any way from that during the fixation pauses.

In any complex field of vision, the successive stimulation of each point of the retina by its various parts must tend to even up the total stimulation of those parts affected, and to break down the sharper contrasts of the fixation pauses. This is essentially a process of equalizing retinal excitation and of preparing for a new fixation.

Moreover, it must be remembered that the duration of the eye movements is very short, for movements of 10° certainly not more than $60\,\sigma$, and possibly less than half that. It will be seen at once that the equalizing stimulation during the movement of the eyes scarcely covers the duration of the most pronounced after-effect of the intense stimulation at the preceding fixation pause; while the interference of the intenser stimulation of the succeeding fixation would make the faint stimulation

hardly perceptible, even if it had no after-effect of the previous fixation, and no central inhibition to overcome in establishing itself.

There is strong evidence, with the details of which this article may not be burdened, that the equalization of the stimulation of each reading pause by the fusion during eye movement to the next fixation point is a very essential condition in the elimination of fatigue in reading, and as well in the prevention of error.

MENTAL FATIGUE. I.

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This paper is intended to be the first of a series of reports of a study of the nature of mental fatigue, some of the conditions affecting it, and especially its phenomena in school life. I hope also to discuss the methods and results of the many workers in this field in the light of some of the conclusions apparently justified by this study.

It seemed wise not to undertake any statistical investigation of fatigue in school children until the fact of mental fatigue had been carefully studied at some length in a few cases. The present section deals, therefore, entirely with the results attained by a study of subjects whose mental history during the experiments the investigator knew, whose method of work he could control and whose phenomena he could therefore interpret with some degree of assurance. It will also be wise to reserve any references to the work of others (of whom Professor Kraepelin is the most important) until these new data are presented.

The present paper may best be introduced by a quotation from a preliminary notice printed in *Science*. "Mental fatigue may mean either the fact of incompetency to do certain mental work, or a feeling of incompetency which parallels the fact, or the feeling or feelings denoted by our common expressions 'mentally tired,' 'mentally exhausted.' Among the conclusions to which the experiments have led are the following: First, that the fact of incompetency is not what it has been supposed to be; second, that there is no pure feeling of incompetency which parallels it and is its sign, that consequently the mental states ordinarily designated by the phrases mentioned are not states made up of such a feeling of incompetency but are very complex affairs, and third, that these mental states are in no sense parallels or measures of the decrease in ability to do mental work.

We have been accustomed to think of mental work in terms of mechanics. The mind has been supposed to lose its power to work as a rubber ball loses its power to bounce. As the ball rebounds to a lesser and lesser height, so the mind has been supposed to think with less and less vigor. We have talked as if sleep charged the mind with mental energy as a current might charge a storage battery with electricity, and that then the mind had this stock to spend. As it spent it, it could exert less and less energy in its thinking. According to such a view, after enough work has been done to get up a momentum, the amount of work the mind could do should steadily decrease in proportion of some sort to the amount it had done since its last rest. And the practical corollary (of tremendous importance) to such a theory would be that, again barring the preliminary start which acts as a stimulant, a man was always mentally exhausting himself, that in every day's mental work we normally approached mental debility, that mental work brought the curse of mental incompetency as its constant shadow, large when it was large, small when it was small. One would then be prevented from doing much mental work when one's mind needed rest simply by being unable to do it.

Another theory would be that mental work had no such constant warning attendant, but that certain extraneous results sufficed in the realm of natural mental hygiene to prevent a man from working when he needed rest. Just as a muscle is kept from working, not by a mere decrease in the power of its tissues, but by the presence of certain poisons formed as by-products, so the mind may be kept from working not by mere inability to work so hard, but by certain outside influences, *e. g.*, feeling bored, sleepiness, headache, which are results of its activity. If the latter were the case, the inhibition of work would probably come by fits and starts, as these outside influences reached summation points, became poison doses, so to speak, and would not come in proportion to the work done. A man might work hard mentally for say eight hours and do just as much the last hour as the first. The curve of ability to work during the progress of work might be not a line like *A* but one like *B*, where the various sharp drops show the influence of the by-

products of mental work. It might also be that we could, perhaps not at all wisely, in some way inhibit the influence of these by-products, and then find ability to do mental work undi-

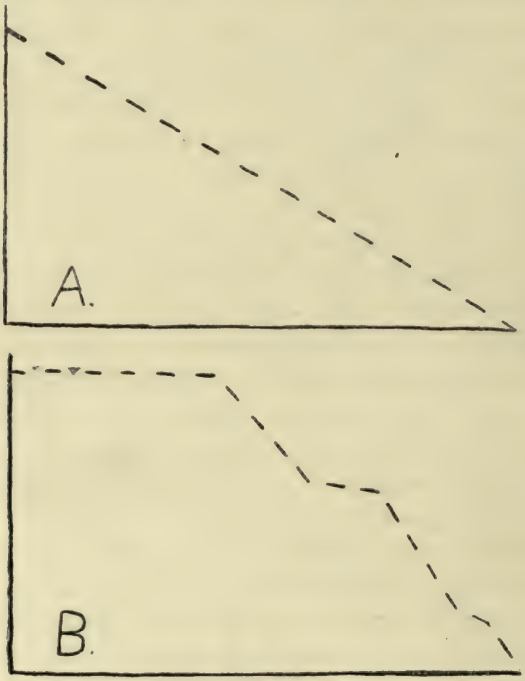


FIG. 1.

minished to any serious extent by the fact of a large amount having been done without rest.

This question seems fairly important, and is of course capable of being settled by experiment. The experiments to be here reported do not settle it, but their results, I think, strongly favor the latter theory.

The method was to test the ability to do mental work before and after periods of hard mental exercise. The only strictly novel test of mental ability was mental multiplication of numbers of several figures each by one another. The results from this test I report first because it seems a rather good one. It is very hard work, at least it was for the subjects chosen, involves an unremitting and intense concentration of the attention, and

also rapidity and accuracy of the associative processes. The very slightest relaxation or confusion probably greatly affects the speed and accuracy of the work. A very little inability to work would, one would suppose, show itself at once in the results.

The first experiment consisted in having a subject who had done five or six such examples, do two a morning on four mornings after good sleep and about an hour's general mental activity, and, after doing hard mental work for from seven to twelve hours, to do two examples each evening. In a second series the examples given in the evening in the first series were done in the mornings, and those of the mornings of the first set were done in the evening. We thus get sixteen such examples, each done once before a day of mental work and once after, and so arranged that practice favors neither side. Nothing of any particular example was ever remembered on any second trial with it.[1] In each case the subject looked at the numbers to be multiplied ten seconds, then closed his eyes and when he had the complete answer wrote it down. The time taken and the errors in the answers were recorded. In every case the evening examples represented a state of mind which had led the subject to stop work on the ground that he was fit to do no more. To start the work of the experiment was very irksome and required some exercise of determination. The record follows. The subject was myself.

TABLE I.

Subject T. Mental multiplication. *Set A*, given *before* work in the first series and *after* work in the second series of tests.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
a.	370 secs.	190 sec.	0	1
b.	215	360	0	1
c.	345	300	1	0
d.	235	340	0	0
e.	300	340	0	1
f.	345	255	0	0
g.	300	380	1	0
h.	440	200	1	0
Total time	2550	2365	Total mis., 3	3

[1] The following are representative examples: 762 x 649, 346 x 782, 279 x 864, 964 x 728.

Set B, given *after* work in the first series and before work in the second series of the tests.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
i.	420	285	0	1
j.	310	340	0	1
k.	205	450	0	0
..	205	255	0	1
m.	215	250	0	2
n.	260	195	0	1
o.	400	305	0	0
(97 × 68) p.	65	60	0	0
Total time	2080	2140	Total mis., 0	6
Total for both sets.	4630	4505	3	9

The next table records the results of a similar experiment with a different subject, a college freshman, 16½ years old.

TABLE II.

Subject E. T. Mental multiplication. *Set A*, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
a.	4 m. 30 sec.	2 m. 45 sec.	0	0
b.	3 " 36 "	3 " 10 "	0	2
c.	3 " 45 "	2 " 15 "	1	1
d.	5 " 40 "	4 " 40 "	0	0
e.	4 " 20 "	3 " 00 "	6	0
f.	4 " 30 "	3 " 24 "	3	0
g.	3 " 10 "	2 " 06 "	2	1
h.	2 " 36 "	2 " 12 "	4	2
Total time	30 m. 37 sec.	23 m. 32 sec.	Total mis., 16	6

Set B, given as in Table I.

Examples.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
i.	3 m. 30 sec.	4 m. 15 sec.	1	1
j.	4 " 12 "	3 " 20 "	0	2
k.	2 " 45 "	4 " 40 "	0	0
l.	3 " 20 "	3 " 02 "	4	0
m.	1 " 50 "	4 " 00 "	6	6
n.	3 " 00 "	3 " 20 "	0	2
o.	2 " 15 "	3 " 24 "	1	3
p.	40 "	1 " 30 "	1	1
Total time	19 m. 42 sec.	23 m. 31 sec.	Total mis., 13	15
Totals for both sets.	50 m. 19 sec.	47 m. 3 sec.	29	21

The mental work done on the days when tests were taken was perhaps not so severe in this case. It ranged in amount from five to twelve hours, and averaged nearly eight hours.

The next table (No. III.) records the results of a similar experiment with a little girl eleven years old as subject. The examples were in this case done before going to school in the morning and immediately after return from school in the afternoon, *i. e.*, after an interval of seven hours, five of which were spent in school. The examples in this case were easier, the following being representative: 42×18, 45×56, 74×47, 87×69. Four were given in each experiment instead of two. The subject had previously done eight such examples, though somewhat easier ones.

TABLE III.

Subject M. T. Mental multiplication. *Set A*, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work.	Done after work.	Done before work.	Done after work.
1, 2, 3, 4	8 m. 32 sec.	5 m. 10 sec.	1	0
9, 10, 11, 12	7 " 57 "	5 " 30 "	0	2
17, 18, 19, 20	10 " 00 "	9 " 04 "	0	1
Total time	26 m. 29 sec.	19 m. 44 sec.	Total mis., 1	3

Set B, given as in Table I.

Example.	Time Taken.		Mistakes in Answer.	
	Done before work	Done after work.	Done before work.	Done after work.
5, 6, 7, 8	7 m. 30 sec.	9 m. 40 sec.	0	2
13, 14, 15, 16	5 " 28 "	6 " 25 "	4	4
21, 22, 23, 24	7 " 18 "	9 " 00 "	1	8
Total time	20 m. 16 sec.	25 m. 05 sec.	5	14
Totals for both sets.	46 m. 45 sec.	44 m. 49 sec.	6	17

The interpretation of these data will come in more suitably after certain others have been presented. As regards the method of solving the examples, each subject worked at the utmost speed consistent with conscious accuracy. In all cases feelings of boredom or fatigue were reported at the start of the late experiments.

In the case of the last subject, M. T., it may well be that these feelings caused less effort. In the case of the other two, so far as introspective results enable one to judge, these feelings seemed to be set aside and the work seemed to be done as well as possible.

To supplement these tests, examples in addition were given after rest and before work and after hard work before rest. The following is a fair sample of one of these examples:

95799
86967
32687
84799
95976
34797
97864
98945
87834
68792
79867
88896
97745
39799
48970
89043
67354
54628
91176
90251

No two were used alike.

As in the multiplication examples, the subject worked as fast as he possibly could without consciously making mistakes. The time taken and the number of mistakes in the answer were recorded in each case. Table IV. shows the records for myself. It will be observed that the records vary very much in point of speed. That is because during the period covered by the experiments I was doing similar examples for other reasons. Of the exact amount of this practice at each date I have no record. But the reader can be sure that neither side was much favored by it, though the 'before work' ones may have had enough advantage here to account for their superior quality. The

mental work on the days when evening tests were taken was approximately the same amount as in the multiplication experiments of Table I.

TABLE IV.

Subject T. Addition.

Examples.[1]	Date.	Time Taken.		Mistakes in Answer.	
		Done before work.	Done after work.	Done before work.	Done after work.
a.	Feb. 26.		60		1
b.	" "		55		2
i.	Feb. 28.		95		0
j.	" "		114		0
f.	Mar. 3.		94		0
d.	Apr. 25,		75		0
k.	" "		70		2
c.	May 3.		55		2
l.	" "		63		1
4	" "		73		0
k.	Mar. 1.	72		3	
l.	" "	98		1	
c.	Mar. 3.	63		0	
d.	" "	78		2	
f.	Mar. 4,	70		0	
4	Mar. 7.	88		0	
a.	May 4.	45		0	
b.	" "	35		0	
i.	May 24.	50		1	
j.	" "	60		0	
Totals		659 secs.	754 secs.	7 mist.	8 mist.

Table V. shows the results of a similar experiment with Miss K., a college senior.

Besides the mental multiplication and the addition, the speed and accuracy of the perception of small differences was used as a test with Miss K. and T. (myself). Twenty-four small cards were used, each having on it a line. These lines were of lengths from 15 to 30 millimeters, sixteen of them forming an arithmetical progression and the other eight being duplicates of some of them. The subject was required to place these in order according to the length of the lines. Record was kept of the time taken and the number and degree of misplacements.

[1] a and b were examples of only 60 figures instead of 100.

TABLE V.

Subject, Miss K. Addition.

Examples.	Date.	Time Taken.		Mistakes in Answer.	
		Done before work.	Done after work.	Done before work.	Done after work.
a.	Mar. 1.		3 m. 30 sec.		0
b.			2 " 00 "		0
c.			4 " 00 "		2
d.			3 " 45 "		0
e.	Mar. 1.	2 m. 45 sec.		2	
f.		4 " 00 "		3	
g.		3 " 00 "		1	
h.		3 " 45 "		1	
i.	Before work. Mar. 19.	1 " 45 "	3 " 00 "	0	0
j.		1 " 50 "	2 " 30 "	0	0
k.	After work. Mar. 2.	1 " 35 "	3 " 00 "	1	2
l.		2 " 00 "	3 " 30 "	1	0
m.	Before work. Mar. 1.	2 " 00 "	2 " 05 "	0	6
n.		2 " 00 "	3 " 00 "	2	0
o.	After work. Mar. 18.	2 " 10 "	2 " 00 "	1	1
p.		2 " 05 "	2 " 10 "	0	1
q.	Before work. Mar. 20.	2 " 10 "	2 " 45 "	2	0
r.		2 " 25 "	3 " 10 "	1	4
s.	After work. Mar. 4.	1 " 50 "	2 " 30 "	1	1
t.		1 " 45 "	2 " 05 "	0	1
u.	Before work. Mar. 5.	2 " 05 "	2 " 05 "	1	1
v.		2 " 10 "	2 " 30 "	0	0
w.	After work. Mar. 19.	2 " 10 "	2 " 45 "	1	1
x.		2 " 05 "	2 " 20 "	0	1
Totals		45 m. 35 sec.	54 m. 40 sec.	18	21

NOTE.—It will be observed that the 'Before work' tests have a slight advantage in point of practice. Subject K. had pains in her eyes in the evening frequently.

Miss K. did this before work on March 3d and March 4th with the following results:

	Time.	No. of Displacements.	Total Displace'ts.
March 3d	300 sec.	7	9
" 4th	260 "	8	9

On March 14th and March 15th the same subject did the same thing *after* the day's work. The record is:

March 14th	300 sec.	6	6
" 15th	280 "	5	6

The subject T. did it before work February 28th with the following results:

	220 sec.	7	9

After work, April 25th, with the following results:

	344 sec.	5	5

The experiments recorded so far have shown the influence or lack of influence of mental work of a general nature on the ability to do mental work of a particular sort. In order to get additional data on this point and also some data about the influence of mental work of a particular sort on the ability to do that *same* sort of work, experiments of another sort were conducted. In these the subject did for some time mental multiplication or ordinary addition, and the rate and accuracy of his work in the early part of the time was compared with that of his work during the latter part. In some of these experiments data to compare with those already presented were gained by giving simple tests of general ability to do mental work before and after the special work.

Table VI. presents the results of two and one-third hours' unremitting work at mental multiplication, the numbers to be multiplied being of four figures *each*, *e. g.*, 8967 × 4532. I., who was the subject, had previously had the practice of experiment 1. The experiment began about 3 P. M. I had worked three and a half hours in the forenoon, and twenty minutes just before starting the experiment. For certain reasons my emotional tone was very pleasant.

TABLE VI.

Subject T.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
	min. sec.			min. sec.	
1	12.10	2	6	17.30	2
2	10.20	1	7	15.50	2
3	19.50	0	8	14.20	0
4	12.40	1	9	11.50	0
5	16.00	0			

It will be observed that whether one compares the first fourth, third or half of the work with the last fourth, third or half respectively, one finds only very slight differences. Thus, the first two examples took 22 min. 30 sec. with 3 mistakes, the last two 26 min. 10 sec. with no mistakes; the first three 42 min. 20 sec. with 3 mistakes, the last three 42 min. with 2 mistakes; the first four 55 min. with 4 mistakes, the last four 59 min. 30 sec. with 4 mistakes. Yet the effort of holding the at-

tention to this work during the latter period was to consciousness considerable. The later examples were to persons doing them out on paper of the same difficulty as the earlier.

Table VII. records the results of a similar experiment with numbers of four figures each, the subject being E. T. who had had the practice in multiplying mentally shown in Table II. This subject proved very capable at this work, and it was perhaps not so hard for him as for T. Though he did ten examples of the same difficulty as those of Table VI., he worked only one hour and ten minutes at it. He started at 8:30 A. M., after eleven minutes of work.

TABLE VII.

Subject E. T.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
	min. sec.			min. sec.	
11	6.45	2	18	6.40	1
12	5.45	4	19	6.40	0
13	5.30	1	22	7.00	2
15	7.20	1	23	6.00	0
16	7.20	1	24	7.00	2

The first five examples thus took 32 min. 40 sec. with 9 mistakes, the last five 33 min. 20 sec. with 5 mistakes. The totals for the first four and last four are 25.20, 8 mistakes, and 26.40, 4 mistakes; for the first and last three they are 18 min. with 7 mistakes and 20 min. with 4 mistakes.

Table VIII. shows the results during 1 1–5 hours of unremitting addition. The examples were of the same difficulty as the one printed. The subject was T. (myself). I had had the practice shown by Table IV. up to March 7th and its preliminary, and had also done some eight of these examples for another purpose. I had worked four hours before luncheon, and did this experiment because I felt unable to do any regular work. The work was begun at eight minutes of three after thirty minutes of other work. Luncheon and a rest filled the time from 12.30 to 2.20.

If we compare the work of the first eight examples with that of the last eight, the totals are 15.25 with 4 mistakes for the

TABLE VIII.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Example.	Mistakes in answer.
	min. sec.			min. sec.	
5, 6, 7, 8,	8.10	2	25, 26, 27, 28	6.45	3
9, 10, 11, 12	7.15	2	29, 30, 31, (32)[1]	5.56[1]	0[1]
13, 14, 15, 16	6.25	2	2, 3, 4	5.15	1
17, 18, 19, 20	7.20	2	103, 104	3.30	0
21, 22, 23, 24	8.50	0			

former, and 14.41 with 1 mistake for the latter. Taking the first and last twelve we have 21.50 with 6 mistakes and 21.26 with 4 mistakes. Taking the first and last sixteen we have 29.10 with 8 mistakes and 30.16 with 4 mistakes.

On March 28th after three hours' work and an hour and a half for luncheon and rest I did eight examples of this same sort at intervals during an hour and a half of mental work (computing errors in experiments and recording results). The records are as follows:

TABLE IX.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
e.	105 sec.	0	t.	95 sec.	0
d.	83 "	0	p.	70 "	0
f.	80 "	0	u.	80 "	0
s.	85 "	0	x.	80 "	1

Beginning at 9.20 P. M. of the same day after a rest from 3 P. M. to 9 P. M., I did at intervals during an hour of mental work nine more examples. The records follow:

TABLE X.

Subject T. Addition.

Example.	Time taken.	Mistakes in answer.	Example.	Time taken.	Mistakes in answer.
r.	80 sec.	1	o.	80 sec.	0
w.	95 "	1	k.	75 "	2
v.	84 "	0	g.	72 "	0
m.	90 "	0	h.	75 "	0
n.	90 "	1			

[1] The record of 32 was lost, so that I have taken as the time for 29, 30, 31, three-fourths of the time recorded for doing all four.

Comparing the first four with the last four we have 5 min. 49 sec. with two mistakes and 5 min. 2 sec. with two mistakes.

Before and after the mental work recorded in tables VI., VII. and VIII., various tests of ability to work were given. A statement of the results they manifest will complete the presentation of the data concerning the amount and manner of influence of work done on the ability to do work.

Before the mental multiplication of Table VI., addition examples q, r, s and t were done, and after it examples u, v, w and x. The record of the early set is 422 seconds with no mistakes; of the late set 407 seconds with 7 mistakes. Before and after the mental multiplication of Table VII., E. T. did example 1 (addition) and sorted the lines mentioned on page 457. After the mental multiplication he did example 2 and again sorted the lines. The records are for the early addition 3 min. 45 sec. with 2 mistakes; for the late 3 min. 30 sec. with 1 mistake; for the speed and accuracy of perception the early test took 4 min. 45 sec. with 8 mistakes, involving ten degrees of displacement; the late test taking 3 min. 50 sec. with 10 mistakes, involving ten degrees of displacement. Before and after the addition work of Table VIII., subject T. was tested for the time of recognizing and naming the cards in a pack, for the time of sorting a pack into piles of aces, deuces, etc., for the speed and accuracy shown in marking on two pages of practically equal difficulty all the words containing the letter e, and for the speed and accuracy of mental multiplication.

The results were as follows:

	Before work.		After work.	
Recognition and naming.	65 sec.	0 mist.	56 sec.	0 mist.
Sorting cards.	120 "	0 "	104 "	0 "
Multiplication.	462 "	0 "	695 "	0 "
Marking words.	180 "	161 marked, 6 omitted.	173 "	160 marked. 2 omitted.

To judge what part the practice of the first trial played, the subject seven weeks later performed similar tests, but separated this time by a period of rest. The times for recognition were 56 and 56, for sorting the cards 110 and 106, for marking words 165 and 158.

Now the conclusion which all these data strongly suggest is that the fact of relative incompetency is not to be explained as the result of a decrease of mental energy in general. Mental energy, if it means anything, must mean a something which mental work uses up in regular proportion to the work done. But incompetency, mental fatigue, *does not come in regular proportion to the work done.* In general it comes very much more slowly. A great deal of mental work can be done with very little change in the ability to work. Thus Table I. shows that although there were over 400 chances to make mistakes in the associations involved in multiplying, besides all the chances to forget the figures (about 18 per example) and the positions of each of those figures, yet only six more were made in the work done late than in that done early, and the work was done *more* quickly. Table II. shows no decrease whatever in the ability to work due to the work done. Table III. shows that with over 300 chances to make mistakes in associations and corresponding chances to forget the figures and their positions, only eleven more were made in the late set, and *less* time was taken. Table IV. *may* show nothing more than the change in ability due to practice or a slowness in recognition due to the inferiority of artificial illumination, or the physical fatigue of the sense organs. If these factors play no part, there is a clear, though slight, decrease in speed and accuracy. Table V. shows a clear decrease in speed, which, however, may be referable to difficulty in vision during the late tests. Tables VI., VII., VIII., IX. and X. show that the decrease in energy does not have enough influence to outweigh the influence of practice.

We see also that the change in ability to work is not in any regular proportion to the work done. If, for instance, E. T. in test 11 had worked twenty-four hours instead of eight, he would in some cases undoubtedly have been entirely unable to multiply three figures by three mentally. He would have fallen asleep or become mentally confused or have failed to keep in mind the first half of the work while doing the second half. Eight hours' expenditure of mental energy had no influence on his ability to do the work, while twenty-four hours would have almost destroyed it.

The fact seems to be that mental fatigue is a very complex affair; that it is not a simple matter of less mental energy but a name that stands for slowness of associations, inaccuracy or inappropriateness of associations, lack of inhibition or irrelevant ideas, interference with purposive trains of thought by feelings of ennui, of bodily pain and strain, thoughts of pleasant activities, impulses to stop work, mental confusion, sleepiness, physical fatigue of the eyes, and perhaps a number of other things. It seems to the present writer that the most hopeful way to attack the problems of mental fatigue is to resolve the fact into such elements and to study each by itself. For instance, the influence of certain kinds and amounts of work on the speed of associative processes is easily estimated. And so with the influence of work on the other factors. Until one so discards the notion of general mental fatigue, one's questions and results are largely meaningless, for they are so vague. Fatigue of a muscle is a definite thing, viz, relative inability to overcome resistance, but fatigue of the mind means inability to do the right *kind* as well as the right quantity of work, and may refer to sonnet-writing, logical theorizing, sharpness of observation or a hundred other different sorts of things.

So much for the *fact* of fatigue. Our second conclusion that there was no pure feeling of general mental incompetency is, of course, the result of personal introspection, though under test conditions. When I tried to analyze my feelings during states which in accord with the social consciousness I called feelings of mental fatigue, of inability to do mental work, I found in them emotions of repugnance at the thought of certain forms of mental activity, amounting sometimes to a sort of mental nausea; feelings of dulness or stupidity (by which names I mean a state of unsuggestiveness, of insipidity), cravings for certain familiar forms of mental relaxation, feelings of sleepiness, heavy feelings in the head, pains in the chest or back (from leaning against a table and sitting upright during work) and sometimes a feeling best characterized by the awkward phrase mental 'goneness,' which reminds one of the feeling of physical faintness. This last is most likely the supposed feeling of incompetency, but I get it only very rarely and not

necessarily after especially hard mental work. I fancy that it has some direct physical cause. I was constantly surprised to find myself when feeling, as I would certainly have said, 'mentally tired,' unable to demonstrate in the feeling anything more than an emotional repugnance to the idea of doing mental work. On at least half the occasions this seemed to be all there was.

If this account of the feeling of fatigue is correct it corroborates the previous account of the fact of fatigue. If the latter were the complex of conditions we supposed, we should not expect that there would be any pure feeling of mental inability. The introspection on which I have based the account may, however, have been influenced by suggestion, and prejudiced in favor of the theory involved.

Our third conclusion was that the feelings of fatigue, such as they were, were not measures of mental inability. Kraepelin has emphasized this fact, but chiefly in connection with the claim that we can be mentally fatigued without feeling so. What I wish to emphasize is that we can *feel* mentally fatigued *without being* so, that the feelings described above serve as a sign to us to stop working long before our actual ability to work has suffered any important decrease which an experimenting psychologist could measure and use as a warning to us. The greater number of the 'after work' tests described in this paper were undergone when the subject would, if left to himself, never have engaged in mental work. He did them often *just because* he felt incapable of mental work. I have myself when feeling thoroughly tired summoned up courage enough to start the repugnant work of multiplying and found that I did it just as well as ever. The young subject E. T. noted this independently and wrote me, "Mental fatigue is all a fraud. I don't want to do the things at night at all but I can do them just as well." Children in the schools where I gave tests early and late in the day agreed that they were tired in the late hours and thought that they couldn't work nearly as well, yet these same children *did* do just as well in the tests given. The relation between the feeling and the fact of fatigue then is not a simple relation of direct dependence but a very involved rela-

tion or series of relations. These feelings may be in part warnings, preventatives of overwork, provided as a beneficent mental adaptation. They may be in part mere useless by-products of our thinking to which mental hygiene need pay no attention. They may even be in part mere accidents of thinking which habitual disregard may abolish. To find out just which of them belong in these or other categories would be a worthy task, for the wise expenditure of one's mental effort depends largely on the way one interprets these symptoms.

The gist of this whole article, then, is the claim that certain current ideas about the fact and the feeling of mental fatigue, and the relation between them, are naïve abstractions based on a simple-minded analogy, a failure to carefully analyze certain mental states, and a confusion in the case of experimental investigations between lack of *desire* and lack of *ability* to work.

A TYPE OF PENDULUM CHRONOSCOPE AND ATTENTION APPARATUS.

BY PROFESSOR JOHN A. BERGSTRÖM.

Of the pendulum chronoscopes which have been constructed for psychological purposes successively by Professors Sanford,[1] Fitz,[2] Scripture[3] and Seashore,[4] the one here described was more especially suggested by the Fitz-Scripture type. Like this it secures the registration of the intervals by the mechanism of a heavy pendulum carrying a light index, which may be clamped at any point of a scale graduated into hundredths to thousandths of a second.

Aside from the circular mercury cup attachment and a few details of construction, it differs from it chiefly in the mode of carrying and clamping the index. The mechanism of this part is exhibited in cross section in figure I, and in a front view in figure II. The same parts in the two figures are referred to by the same letters.

The steel axis $X^1X^2X^3$ is cut so as to leave the solid sectors K^1 and K^2, which have an angle of about 30°, in line and with knife edges coincident with the longitudinal center of the axis. These knife edges rest in shallow V-shaped grooves of hardened steel on the vertical supports V^1, V^2, and the parts are so proportioned as to allow the pendulum to swing through the necessary arc without jolting. The axis, which rotates as it would on any other centered bearings, is cut down to a smaller diameter in the forward projecting section X^1 and carries there

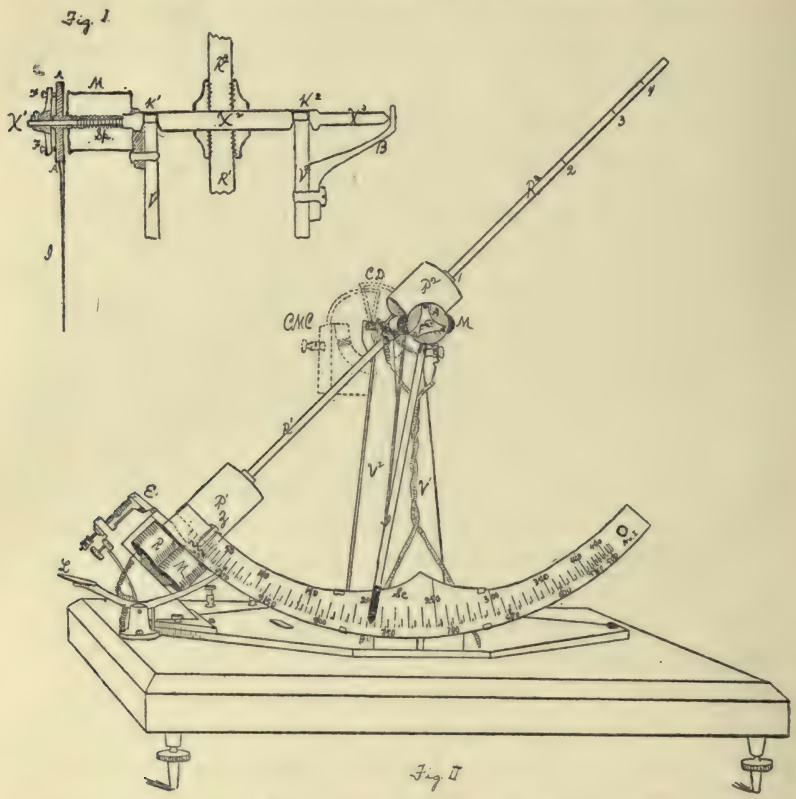
[1] E. C. Sanford, A New Pendulum Chronograph, *Am. Journal of Psychology*, Vol. V., No. 3, April, 1892. Also, The Vernier Chronoscope, Vol. IX., Jan., 1898.

[2] G. W. Fitz, A Location Reaction Apparatus. PSYCHOLOGICAL REVIEW, 1895.

[3] E. W. Scripture, Studies from Yale Psychological Laboratory, Vol. III., 1895.

[4] C. E. Seashore, Spark Chronoscope, University of Iowa Studies in Psychology, Vol. II., 1899.

the essential parts of the recording mechanism. This consists of the circular armature *AA*, which may slide forward and



backward or rotate freely on the axis X^1 and has the index *I* attached to its circumference. The spiral spring *Sp* holds it against the tripod *F*, which serves to adjust it to the proper distance from the electromagnet *M* and to provide the friction necessary to carry the index along with the pendulum as it swings.

If a current of sufficient strength is sent through *M*, the circular armature *A* is at once attracted and moves from the friction tripod *F* to the magnet, by which it is held immovable as long as may be desired—the pendulum, in the meantime, swinging on with little resistance till it is caught on its return by one of the lower teeth of the release armature *E*. The

backward pressure exerted by the spring *Sp* on the axis when the armature *AA* is thus clamped is checked by the brace *B*. The lever *L* serves to move the pendulum up to its starting point, while the index is set back to zero by moving it against the projecting pin *Z*.

The time of a process is measured by sending one current through the release magnet *RM* at its beginning; and another through the index magnet *M* at its end. Both the release of the pendulum and the clamping of the index armature are thus brought about by the closing of the circuits; and the currents may always be kept in the same direction, so that there is no danger of any error from residual magnetism.

This, of course, does not necessitate the use of make-circuit keys in experimentation, but break-circuit keys may be employed as well, if they are used as shunts in the circuits of the magnets. The latter method should, in fact, be adopted in all work requiring a high degree of accuracy on account of the somewhat variable lengthening of the time, due to make-circuit keys. Break-circuit keys, however, while less objectionable, are not wholly so, as variations may be introduced by different tensions of the spring acting on the lever, and the rate and force of the movement of the arm.

In constructing the scales, use was made of the well-known spark method for registering intervals. A fine wire was run down the index so as to leave a point projecting at its end to within a millimeter or less of the metal plate on which the scale is fastened. The front surface of the metal plate was then covered with smoked paper moistened so as to adhere closely, and the sparks from an induction coil interrupted by a hundred-vibration tuning fork with a mercury contact were allowed to run over it as the pendulum swung from one end to the other.

Since the pendulum might start at any point between two sparks, successive spark tracings would not coincide, and the number of sparks up to any point on the scale would not, except by chance, represent the absolute time. Moreover, it would not be possible to construct the scales for the return swing, of which use is made in this instrument, on account of the confusion of records. To obviate the trouble and secure the ad-

vantage of successive coincident tracings, five or six of which were imprinted on the copies for the scales made here, the starting of the pendulum was made dependent upon the dipping into the mercury of the same vibrating point of the tuning fork that broke the circuit of the induction coil for the sparks. By reducing the depth of the dip into the mercury the time between the making of the circuit which releases the pendulum and the breaking of the circuit which gives the spark may conveniently be made about two thousandths of a second, an interval for which compensation can easily be made by the mechanism of releasing the pendulum and clamping the index so that the instrument will register the absolute time.

When the forward scale has become well fixed by half a dozen tracings and has besides been marked, the return scale can be constructed without confusion.

Relative variations in the strength of the currents operating the two magnets produce variations in the records. The instrument must therefore be adjusted for a certain ratio, and the most convenient is, perhaps, that of equality. If the currents are equal, considerable differences in the absolute strength, as from one-half to one and one-half ampères, do not seem to affect the record appreciably, though a current of one ampère is usually employed.

The chronoscope has four scales numbered from one to four and corresponding with times of vibration of about one, one and one-half, two and three seconds. The different times of vibration are produced by placing the counterpoise P^2 at points 1, 2, 3 and 4 on the rod R^2. For the usual experiments upon the duration of mental process, scales 1 and 2 or 4 have been found most serviceable. With 4 the forward scale is usually sufficient for the longer reactions, but the return scale of number 2 by giving greater distances for the intervals can be read with greater precision and may be used for the same purpose, and there is less difficulty in securing the proper adjustment of the counterpoise. The counterpoise is eccentric with respect to R^2 so as to make it possible to give the pendulum the proper balance in each case; and upon this and the proper leveling of the base depends the securing of the absolute time.

For regulating and learning how to manipulate the instrument use was made of a falling ball apparatus, like one described several years ago by Professor Jastrow.[1] In this, an iron ball about three-fourths of an inch in diameter is held suspended by a short, straight electro-magnet. On the interruption of the current the ball falls and is made to strike a break-circuit key, which may be placed at different distances below the magnet. This break-circuit key serves as a shunt in the circuit of the index magnet, so that opening it leads to the clamping of the index. The time required by the ball to fall to the key is then compared with the readings of the chronoscope. To make the release of the ball and the starting of the pendulum simultaneous presents considerable difficulty, aside from that due to the inertia of the magnet; and two kinds of keys were used for the purpose, one like that constructed by Professor Jastrow with double levers, in which the striking of the first upon the second makes one current, while the lifting of the second from the impact of the first breaks another, and a second in which two currents were broken by the lifting of the same long lever from two insulated points of contact. The second approximates most closely to simultaneity, as the intervals recorded are about .001^{s} longer when the first is used, and the mechanism is such that the circuit for the release of the pendulum is made first in the double lever key.

The manipulation of these keys represents one of the chief sources of error, as small differences could be produced by varying the rapidity of release.

At points in the scale where the thousandth of a second was represented by about a millimeter, and the scale was so marked, the readings in tens of thousandths being estimated, the writer found that the probable errors of single records deduced from groups of fifty successive records each were for intervals of about .200 and .300^{s}, $\pm$ .00021 and $\pm$ .00012 respectively. The difference in the probable errors is perhaps due to improvement in manipulation, as the two groups of records were taken in succession, though small errors in the estimation of the ten-thousandths must occur. The corresponding average

[1] Jastrow, *American Journal of Psychology*, Vol. IV., p. 208.

variations are, .000168^s and .000119^s, and the average variation of the instrument for the two series together is .000144^s.

In comparison, the average variation of the Hippchronoscope tested by Professor Jastrow by the same method was .002^s, or nearly fourteen times as great; and Professor Sanford gives .002^s as the lowest average variation of the pendulum chronograph.

The accuracy of the other instruments is not stated definitely. Professor Scripture says that the average error of the chronoscope constructed at Yale is not more than .002^s, and Professor Seashore, that the value of the average deflection in the striking of the spark in the spark-chronoscope amounts to less than $\pm$ .001^s when the divisions into hundredths equal 5 mm., and varies inversely with the lengths of these divisions.

I am indebted to a group of three students for the following probable errors for single records from groups of fifty records each for the intervals .100, .200, .300 and .400^s with scale I., namely, $\pm$ .00014, $\pm$ .00016, $\pm$ .00022 and $\pm$ .00015^s respectively; and for intervals .200 and .400 with scale II., $\pm$ .00013 and $\pm$ .00024^s, the last group being taken hastily at the end of the period. In this case the estimate into ten-thousandths is, perhaps, less certain, because it was done by the deviation of the point from the center of the dividing lines on the scales, while the scales were marked into only two-hundredths. The correspondence with the previous records, however, is sufficiently close.

Another feature of the instrument is represented by the circular mercury cups *CMC*, and the circular dips *CD*, which are only attached for certain purposes and are consequently represented in dotted outlines. The dips *CD* can be moved around the axis X^3 so as to make or break a circuit at any point of the swing of the pendulum, or to make or interrupt a circuit for different intervals of time. For the measurement of intervals less than .050^s they may be used to transfer the readings to the central part of the scale, but they come especially into play when the instrument is used for the study of attention to simultaneous stimulation of vision and other senses. As in Wundt's[1]

[1] Wundt, *Physiologische Psychologie*, 4th Ed., Vol. II., p. 405.

apparatus, the moving index supplies one stimulus while the others are brought about by electrical means on the closing of the circuit through the mercury cups. The time errors can be determined directly from the scales, though for extensive experiments more scales than the instrument now possesses would be needed.

The use of the mercury contacts is in some respects inconvenient. Without constant care the accumulation of mercurous oxide and other materials, and to some extent the lack of constancy in the surfaces, particularly on breaking contact, will give rise to large variations in the records, and even with fair care the probable errors of the instrument are two or three times as great as when they are not employed. Thus for breaking circuit with the mercury covered with a layer of either alcohol or kerosene, the probable error for a single record in a group of fifty was $\pm$ .0006^s. The addition of the alcohol or kerosene makes the circuit break three or four thousandths of a second more quickly. The contacts were cleaned at the beginning and again at the middle of the series. The probable error in the case of the making of the circuit was $\pm$ .00039^s. The surfaces and dips were cleaned at the beginning and four times throughout the series, but no alcohol or kerosene was used. The variation in the latter case is due not so much to irregularity as to the fact that from one cleaning of the contacts to another there is a progressive lengthening of the record. The average of this group would therefore be quite reliable as the starting point for the measurement of intervals, and a rather high degree of accuracy might be attained by making due allowance in other cases.

The instrument was constructed about three years ago in the workshop of the psychological laboratory of Indiana University, and has proved very convenient. It can be operated more quickly than it is profitable to make the usual experiments.

DISCUSSION AND REPORTS.

PROFESSOR JUDD'S ILLUSION OF THE DEFLECTED THREADS.

In the number of this REVIEW for May, 1898, Professor Judd described a novel optical illusion, the peculiarities of which he regards as confirmatory of a particular doctrine of space perception. So far as I know, Professor Judd's proposed explanation of the matter has thus far stood unchallenged, for the reason, perhaps, that readers of space literature are more and more well disposed towards the general position that his conclusions represent. The essentials of the illusion and its explanation may be briefly recalled. Two threads, lying in different horizontal planes, are stretched across a box in such a way that their intersection forms an acute angle. The line of sight of the observer should form an acute angle with the planes of the threads. Under these circumstances there are obviously two points of intersection of the threads, one for each eye. Alternately closing either eye will reveal the location of these two monocular points. It is in the region between them that the following illusion occurs. With both eyes open, choose a fixation-point midway between the two threads, in about the middle of the region just mentioned. If this point of fixation be properly chosen, two illusory threads will suddenly appear lying at the right and left respectively in *vertical* planes and thus joining the two actual threads. Their slopes are not identical. One is nearly perpendicular, the other inclines strongly down and away. If viewed from the side they would appear in the form of a somewhat distorted X. The explanation offered is this: Each eye alone sees a pair of intersecting threads without being able to perceive any difference of depth between them. For a single eye the threads appear to lie in one plane and cross at a point peculiar to that eye. Binocularly, however, there are differences of depths perceivable. With both eyes open, therefore, there are two monocular points in the binocular field. Accordingly vision is under the necessity of somehow adjusting itself satisfactorily to both sorts of demands, the monocular as well as the binocular. The result is the two illusory threads running through the third dimension and connecting the mon-

ocular crossing points. The theoretical deductions are wholly along the line of emphasizing the importance of *binocular* factors in all visual perceptions of depth. Impressions that are strictly *monocular* have no definite meaning in terms of distance, whether relative or absolute.

Now while I am in most hearty accord with the general position towards space perception that Professor Judd has expressed both here and in other papers, I cannot see my way clear to accept the connection that he makes between this particular illusion and his theoretical comments. For I have recently become convinced that the experimental analysis of the illusion was, if I understand it, deficient in several critical points. And, unless I am greatly deceived, a much more simple and straightforward explanation than the one originally given must be accepted.

In the first place, inclination of the line of sight to the planes of the threads is not at all necessary. In fact the definiteness of the illusion is perhaps greatest when the line of sight is perpendicular. And, what is vastly more important, *there is absolutely no necessity that the monocular crossing points be in the field of view at all.* Two strips of black cardboard may be so placed upon the edges of the box in which the threads are stretched that the two eyes may clearly see the region between the two monocular crossing points while these points themselves are screened from view. The illusion is now only the more vivid. Yet under these circumstances there can be no thought of a necessary adjustment to two sets of relatively conflicting factors. The cause of the illusion must be sought elsewhere. I find this cause to lie solely in the particular fusions of images which take place when the point midway between the threads is fixated. What happens may perhaps be most clearly described by noting what changes take place in the binocular images as the point of fixation is raised from a position *below* both threads. The following figures represent these successive changes. Let the threads be stretched so that the upper one runs from left to right, the lower from right to left. Let a and b designate the images of the two threads received by the right eye, c and d those received by the left eye. Whether the image comes from the upper or the lower thread will be indicated by the letters u and l respectively. Finally let R and L be the inclusive or single designations for what is seen by right and left eye respectively.

Fig. 1 presents the case where the point of fixation is below the level of both threads. There is no fusion of double images. The pairs ab and cd are upon disparate points, and of course four sepa-

rate impressions are received, the double-images being of the 'crossed' variety. In Fig. 2 the fixation-point is on the lower thread. The images *b* and *c* have united. Crossed double-images of the upper thread remain. In Fig. 3 the fixation-point is raised to the upper

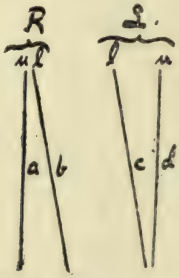


FIG. 1. Fixation-point on bottom of box.

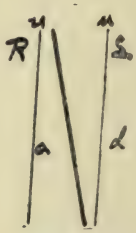


FIG. 2. Fixation-point on lower thread.

line, whereupon *a* and *d* fuse. Uncrossed double-images of the lower line remain. In Fig. 4 is depicted the state of things when the fixation-point is *between* the threads.[1] *a* and *d* have not come near enough to fuse, as in Fig. 3, while at the same time *b* and *c* no longer fall upon corresponding points, as in Fig. 2. The entire process due to the successive fixations is, of course, to be pictured first as an approach, then as an over-lapping, and finally as a crossing over of the two monocular fields. Now Fig. 4 gives the stage of most perfect *over-*

FIG. 3. Fixation-point on upper thread.

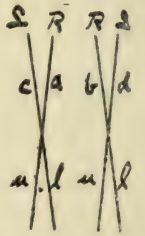


FIG. 4. Fixation-point between lower and upper thread.

lapping. Each image of one eye falls obliquely upon an image of the other. Consequently the images must fuse at and about the points of intersection, for the simple reason that the latter represent correspond-

[1] A movable fixation-point within this region may be conveniently secured by introducing a wire, or something similar, through an opening in the side of the box.

ing retinal points. And just in this state of partial fusion of two linear images,—such fusion that crossed double-images remain on one side of the point of fusion while uncrossed double-images remain on the opposite side,—just in this state lie the indispensable conditions for the perception of lines running obliquely towards or away from the eyes. Any straight line (*e. g.*, a piece of wire viewed against a uniform background) will, when held so as to slope obliquely *towards* the eye, present the right hand cross of Fig. 4. A line sloping *away from* the eye will appear as the left hand cross of the same figure. The double-images existing on both sides of the fused point of fixation are often hard to see, but they are none the less there. Running the point of sight along the perceived direction of either illusory line brings about their progressive fusion, exactly as when the eyes pass along an objective line of a similar slope. And herein, of course, lies the ground for any and all perceived linear *directions*. For any such perceived direction means neither more nor less than that the point of sight must necessarily take this course in order to bring the various binocular impressions of the line upon corresponding points. If this view of the matter is the correct one, nothing but the universal laws of tri-dimensional vision are here in operation. Vision is not adjusting itself to novel conditions supplied by the experiment. It is simply behaving as it always does in constantly recurring experiences of everyday life.

Several further facts are to my mind entirely confirmatory of the position above taken. It will be remembered that, when two rows of retinal elements are in *complete* fusion, the resulting perception is that of a line perpendicular to the line of sight. The necessity for such perception under these conditions was, as the reader will recall, the explanatory basis of the interesting illusion described by Mrs. Franklin in Vol. I. of the *American Journal of Psychology*. In fact the illusion here in question is at bottom only a complication of that. Both involve identical principles. But to return. By properly adjusting the line of sight to the planes of the threads, one of the illusory threads may be made to lie perpendicular to the line of sight. As things were arranged in my own experiments, this phantom thread could be made to appear at the *right*. Meanwhile the phantom thread at the *left* assumed a marked slope. And at the same time this latter thread possessed neither the solid color nor the spatial stability of the former. While the former remained steadily there, the latter tended to break up into its components and vanish. The double images of the latter were too obtrusive, though a proper movement of the point of

fixation could successively unite them and keep the line alive. With the line of sight perpendicular to the planes of the threads—the position recommended above—the slopes of the illusory threads are the same, though in different directions. And in this case both threads are seen by careful gazing to possess double-images on both sides of the region of fusion. In fact one is reminded of the appearance of the lines in the stereoscopic view of a truncated pyramid. The double-images are essentially the same in the two cases.

A still further observation adds a strong bit of positive evidence. An entirely new form may be given the illusion by supplying a new objective condition. Stretch a third thread along the bottom of the box and parallel with its sides. Let it lie midway, laterally, between the monocular crossing points of the original threads. We have now three threads at different levels. Fixate the middle one at the proper point, and the impressions from the upper and lower will unite to form the perception of two phantom threads, similar to those with which we are already acquainted, but longer and with a greater distance between them. Here, as in the original case, the various monocular crossing points may be entirely screened from view.

In view of these various considerations I cannot refrain from the conclusion that Professor Judd's illusion presents no new visual principle. To say that it is due to the necessity of satisfying demands imposed by the presence of monocular factors in a binocular field is hardly more than to give a general statement of the conditions under which the illusion is to be seen. It is certainly not a true *explanation* that is thus offered. But however that may be, I am convinced that we have before us in this illusion only a particularly interesting case of what is eternally happening whenever we open our eyes to see.

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VOLITION AS A SCIENTIFIC DATUM.[1]

The writer would willingly be in error in his apprehension of a certain tendency apparent in recent discussions in psychology, which, if real, is unwholesome in its effects upon the development of the science. In many directions, the effort of listening with obedience to the behest of the age that all knowledge and all doing shall be made 'scientific,' seems to be repeated in as many renewed forms. As psy-

[1] Read before the American Psychological Association, New Haven, December, 1899.

chologists we have been defining our composite results for many a day as the science of mental phenomena, just as though these phenomena could not be considered from any other point of view, or with the aid of any other method than that which psychology uses. But our scientific colleagues have been insisting that this staple characterization can continue only through courtesy on their part. Some of us seem to have been struggling to make their politeness good by accepting the implied criticism of what they deem the unscientific character of psychology. The positive changes due to an acceptance of this hint constitute the apparent tendency in certain quarters to whip the science of mind into a definite shape, thus catering to the respect of the critical colleagues, with the hope of making an exposition of the science seasonable to the current progress of science in general.

The potency and scope of this tendency, described in some such general manner, are shown in the reconstruction of the whole subject-matter which is thereby necessarily involved, and actually carried out in some familiar instances. The postulates of explanation and the principles testing the truths arrived at, the intimate nature of the particular method, or methods (if a plurality can be clearly thought of in this connection), the actual factors regarded as the starting-points for explanation in each typical experience, the specific resorts devised in exposing the processes which effect the results known in terms of composite events in consciousness, are some of the particulars upon which the tendency expends itself. In other words, the truths of the science of psychology are forced into a re-making. The elements of conscious experience are to be reduced to those of one kind or quality only. The laws descriptive of the combinations of elements into the total product of any developing process are to be reduced in number and formulated solely with the terms of the fewer elements. Such simplification in the equipment of the science is supposed to render its ascertainments more truthful than are those which fill the psychology which is to be superseded.

It is the aim of this paper to take up briefly one of the points which is profoundly affected by this tendency and the consequences which appear in its train. It is a discussion of volition as a datum for the psychologist, in so far as he is interested primarily in the processes and functions of consciousness, that is proposed. Specifically stated, it may stand thus: Is the psychologist required to recognize volitional appearances in consciousness as constituting an independent aspect thereof; or, can he be justified in resolving volition wholly into certain forms or elements of consciousness which do not primarily possess the

marks of volition even in the form in which he would thus essentially admit it.[1]

In one respect the phenomena of volition present psychology with one, particular, partially isolated, and rather definitely statable problem. It may be found in a number of expressions. What are those descriptions which enable one to pick out volitions from sensations in consciousness? Does a characteristic term, or do varying terms, sum up its psychical nature? How and when does volition arise? Into what mechanical or organic relations with other alleged psychological data can volition enter? Can volition be regarded numerically, *i. e.*, counted in terms of time-rate, or measured in quantity? To what extent and in what manner is volition to be related to cerebral processes in the two-fold time relation of before and after conscious changes? Is volition to be viewed as a problem for psychology only, or can it be made plain through some other form of approach? However many might be the detailed propositions that set forth the problem of volition, it must be put upon a par with the specific problem of sensation or of movement, and must be solved—in so far as the psychologist solves anything by his manner of relating conceptions of elements and processes—with the aid of conceptions appropriate to itself rather than to other forms of consciousness.

On the other hand, volition and its cognate phenomena can be looked upon as constituting the very question of psychology itself. Here is where the tendency under consideration makes its greatest influence felt. For example, the most interesting book of the year just ended makes psychology get along very well as it is argued, without volition and its derivative phenomena. Through this sweeping negation of certain data, psychology is cast into a form different from that which has prevailed hitherto. Does the ability of psychology to continue to exist as a 'science' depend upon its attitude towards facts of a volitional quality? This relation has, indeed, been urged over and over again, in one way and another, in most of the great treatises which have appeared during the last decade.

It must not be overlooked that many writers who support the tendency under consideration do so while *apparently* recognizing the validity of volitional phenomena. This double attitude expresses

[1] The issue can be best brought to the surface by means of the alternative. This need not prejudice one in favor of the 'either—or,' or of the 'neither—nor.' To psychologize is not to deal in mere logical oppositions; the historic struggles lying behind the present achievements of science have been too great for us to allow ourselves, with any sort of family respect, to relapse into such limitation of childish thought.

itself thus: volitional processes may be spoken of, but always with the insistence that their supposed primary marks are, in reality, only those of a 'developed and secondary' phenomenon in consciousness. Indeed, at a much earlier time, the practical outcome of associationism foreshadowed the current tendency in its reduction of volition to a mere aggregation of "ideas of muscular motion with the ideas of those pleasures which the motion produces." And the tendency at present is well expressed in its determination to keep volition securely within the region of 'secondary' phenomena—and thus rejectable—by analyzing it into elements which do not possess, in separation, those qualities which volition is conceded to bear. The elements reduced to one kind are supposed to do all the work for the student by way of making full and satisfactory explanations.

This brings us face to face with one of the chief demands made by the tendency in its notion of what scientific attitudes are in psychology. Can one be justified in lending his efforts to the support of this tendency to eliminate all data which are regarded by some as not readily conforming to the modalities of scientific explanation? Inasmuch as the dominant term in our theme is 'scientific,' let us pause a moment to get a proper conception of what science involves. When one regards the trained student at work with his object, the activity exhibited may be considered in its four phases of aim, material, method, and result. There can surely be no question as to the relative intentness of the student. He has come into relation with his object through a process of selective discrimination. It is a forced and secondary relation, entered into with the aim of getting some sort of knowledge, which shall at least differ from the casual information incidental to the mere passing daily experience of him who is not such a student. To be a scientist is to maintain an assumed attitude towards the content studied.

Our imaginary student is all the while engaged with some 'object.' It need not necessarily be a center of detachable references objectively regarded. What the object may be must be determined by the student, rather than by ourselves as observers of him. In so far as the description of his object is not in conformity with those given by his associates elsewhere, he can be called to account for his digression. The material in any issue cannot be a fabrication of the moment. It is compelled to carry the credentials which can be given by experience alone. Just as soon as the center of scientific activity is shown to be beyond the limits of a given region, other students with like aims are freed from all demands made by him. Naturalness, recurrency,

sequence, and openness to experience, these, at least, are the primary characteristics of the material involved. Indeed, the whole of our present thought is concerned with this particular phase of the tendency which would limit the possible number of objects to which the psychologist can legitimately turn his attention.

The object of inquiry is not merely possessed for the time being by him who studies it, but its possession must have been gained in a definite manner. Facts lying around loosely, guessed at or gathered by every means, cannot be turned over into the precincts of science. Instant appropriation of that which is given in ordinary observation, and the acceptance of such material as comes in due course of advance to objects that are remote, are scientifically approvable when controlled by the aims already mentioned. Mere method of approach and mere method of inquiry, however, cannot alone constitute the one attitude in question. Here the material has a peculiar dominancy which the most artful student is ever forced to recognize. Facts of one order cannot be converted into facts of another order for the purpose of simplifying methods.

Again, the student fulfills his mission only when giving something as a result of his exceptional activity. Here the circle, so to speak, completes itself as his pronouncements show forth the original aims. The real outcome of science, and so far forth the function of science, is the formation of just those judgments about specific 'objects' in which you and I and the unborn thinker in his day may share alike, in terms of appreciation or of revision. In a large measure, the mark of validity of such judgments consists in their sharability. To put it in another way, the result of science primarily is the ability of the student 'to say things' which he has never said before, and which others may say after him. In this wise, the science of psychology finds its business to consist in getting valid concepts and judgments on the basis of those facts which are contained within its sphere.

If the changes in the science, consequent upon the tendency we are considering, are made in sole deference to the achievement of the well-established physical and chemical endeavors of man, then the tendency mistakes formal appearances in results for real values in fact. Mere imitation never discovered an element nor devised the formulation of a law. If, on the contrary, the apparent departures in psychology pronounce themselves progressive because true science must expound itself after one fashion only, and that the fashion of some one or two definite sciences, then all efforts made for this purpose are duped into the acceptance of an epistemological ground-work that has

no appropriateness with reference to the facts that are to be known or made the point of departure in every scientific development. Mere modality in the thought-world should fill the attention of the rhetorician only.

Let us turn to get a correspondingly brief glimpse at the second important item in our heading. What is it for a thing to be a datum of science? What are its marks? How many different kinds of data is the student justified in discriminating when he proceeds to the selection of those in one group, rather than those in another group? There is more danger to science of every order in the forced recognitions demanded at this point than through any other disposition one can assume, unless it be that which breeds shallowness. The general tendency forcing those recognitions grows, to a large extent, out of that mode of thinking which conceives all attempts at science as under the necessity of reducing results to the 'lowest terms' common to all sciences alike. The very suggestion of our theme is explicit with reference to the dependence of a science upon the character of the material which must be admitted into it. To say that psychology presents one with the merely subjective side of that which is objectively given to physics, chemistry or neurology, is a mistaken conception of their interrelation. It is a position which fails to offer any suggestion of newer methods of inquiry, and at the same time renders obscure a possible insight into the higher unities which manifest themselves in the course of conscious developments. How far, might it be dubiously questioned, would a physicist get with his problems should he suddenly propose to push his solutions thereof into the forms of psychological explanation? That the medley he would thus produce could not be accepted by either the physicist or by the psychologist, one could well guess. And we might even raise strong doubts as tc whether psychology changed under the influence of this tendency is any more acceptable to the exact sciences than is the psychology which deals with mental phenomena in its own characteristic manner.

That which forms the content of the student's labor has been called his 'object,' in the most general sense of that term. But, what marks must the object possess in order to be admitted to scientific cognizance? Over against the reply to such a query should be placed the one given in response to this:—What are the literal characteristics of any datum whatsoever? In either instance, it is needful to recognize the intimate relation obtaining between the facts of experience, as such, and those facts which can serve, in any explanatory manner, the aims of the scientific investigator. The first delimitation

of any science consists in the specification of the qualities which a datum must possess before it can arise in the attention as demanding full value in any description or explanation. Can the object be a 'thing,' spatially or objectively regarded? Must the phychologist's 'object' be described as a mere 'aspect' of that which is actual to the attitude maintained in physics? Can volition, which is not a spatial 'thing' nor a physical 'aspect,' be a datum in the most elementary and simple sense of the word?

After so much epistemology, which seemed unavoidable, partly because the support of the tendency in question is drawn from just such considerations, let us turn to the problem of volition already indicated, and consider the points of regard from which volition may be looked upon as constituting lawful material for the psychologist. By volition, one should understand reference to the active or passive aspects of consciousness, in one way or another. Volition technically regarded as a developed complex in experience, rationally expressed, or as the collocation of conscious contents into a definite adjustment at any given moment, or as the psychical correlate of the physical process known as movement, is apart, to a limited extent, from the meaning carried along in this discussion. However, we are not to search for volition in any 'content' as a mere passive possession in consciousness. This caution should not lead us to the length of thinking psychology to be limited to the treatment of bare material of presentation. Relations are valid 'data' for the scientist; and often these are to be found in processes which are natural, recurrent and exhibit sequences.

The data of psychology are the facts in and the facts about consciousness—in so far as the latter may throw any light upon the former—the constant unit of inquiry always being the total state of mind at the given moment. The facts, when specified in the sequence of conscious content, must be treated in their relations to each other, rather than in their relations to brain-processes, or to the objects regarded as stimuli. In general psychology the concrete data do not recur in the time series, since they include the sequences of history regarded totally. Hence the extreme difficulty of establishing psychological laws of historical and social phenomena.

It is only in the analysis of individual consciousness that one can find recurring phases of experience, as they are given, for example, in the life of a plant,—albeit, a mental state when passed is gone forever. The phases are discovered in comparing a given state of consciousness with an earlier or a later state. To secure any sort of a support for a

denial of this labor as a legitimate task for the science of psychology, one must needs go outside the boundaries of that science itself,—a very dangerous proceeding in view of the logical precedence established thereby. In such a manner volition is found to be constantly recurrent in consciousness. A multitude of successive experiences is required before the subject exhausts all possible forms of sensation, movement or feeling. In any instance of the latter, the immediate content of consciousness is not the form of consciousness in general, but the particular modification as sensation, movement or feeling. The volitional reference in any one of these given cases tends to bear an identity with a previous reference, though limited to the narrowest range of qualitative variation.

This definiteness of the volitional process is greatly in its favor towards accepting it as a datum for psychological purposes. Each volition is specific and definite, giving a marked setting to the content of the mental state,—in fact making the state of mind to be what we actually find it to be when described in relation to its object. This feature of volition is not to be identified or confused with the precision characterizing the sensational content of blue, or the ideational content of an image of the moon or of C-natural; but it is none the less pertinent to the analysis of the process, and in so far forces an enlargement of the conception of the content of the science as to admit it on equal grounds with other data.

There is also a certain universality to volition in mental life which entitles it to a more generous recognition than the tendency is inclined to allow. All forms of mind action give evidence of this process prevailing. It may include in its references all types of sensation, ideas, thoughts, all significant movements, and the more important turns which particular feelings may assume. This disposition of volition to unite with the entire variety of content shows strong evidences that volition is as primitive a factor in conscious experience as are those typical receptive modes of consciousness which are traceable to definite stimulations of peripheral origin. Volition is not a 'receptive' process (excepting the mode of attention, which is generically allied to it), but it is to be considered as more definitely connected with the transforming (intellective) and expressional processes of consciousness. That it is not a static content, one can readily agree, without impairing the originality of its character when contrasted with the composition of such content.

Action is regarded as a proper topic for a scientific psychology. The tendency in question obscures that aspect of volition as a process

in consciousness which is revealed in our inability to understand any one of the physical movements the organism may execute, without first knowing something about active consciousness. If a movement is to have 'meaning,' then it stands as the 'expression' of an idea. Even in cases of purely peripherally stimulated movements, they are described and explained as 'non-voluntary.' The concept of volition is necessary, by implication, to characterize such movements. Furthermore, any study of movement as a sequence of definite changes in spatial relations, cannot be fully carried out without implying reference to what the movement accomplishes, either in the objective or in the subjective events. Each movement thus has a real and particular 'function.' It does something, while involving complex structures. This aspect is deeply implicated in the barest conception of the reflex-arc. Reflex movements are those which occur with special reference to a definitely applied stimulus. The movement's function, however, is not to be identified with or paralleled by the function of the ganglionic centers or of the special sensory-motor tracts which may, perchance, be concerned in maintaining the given reflex. The current tendency is too prone to accept movement itself as the primary psychological datum for throwing light upon the nature of volition as a datum secondary thereto. Movement is but a physical phenomenon, and as a mere succession of changes leaves the problem of volitional analysis as dark as ever. The weakness of the position is further shown by the suggestion that the psychologist is more at home, so to speak, both in matters of fact and of theory, when knowing something first about the volitional structure of any given state of mind. This interpretation can be strongly insisted upon in spite of the ordinary procedure of beginning a psychological inquiry of volition with a study of movement. The prime condition of psychological study is not to be found in the presentation of mere movements to one's senses.

There is a certain bit of instructive analogy along this line, even if it is not conclusive. What a movement may be as mere movement is determined by the structure of the organ or organs involved in bringing the movement about, the varying sizes of the muscular fibers, the tendon and joint attachment, etc. These items of structure do not give the slightest clue as to any possible movement the total organ might perform. That is revealed only upon a basis of perceiving the movement in its actual performance. The movement, as such, when occurring, is functional, not structural, and must be considered as the coördination of multiple 'static factors' into the one complex which the movement itself is. The movement is absolutely novel in the his-

tory of the organism, and herein lies its primacy. When development in movement takes place, or even when a repetition of one movement or a succession of several movements occurs, then the student can compare the movements among themselves and get the several empirical facts about the movements as mere occurrences. The point to this analogy is that the tendency under consideration cannot free itself with aid from this side of conscious experience from regarding volition as definite and independent, even in its psychical aspect of mere movement.

When a class of students in psychology is asked to analyze consciousness with the purpose of getting at the volitional 'content,' or the volitional 'process'—as the case may be, for the analyzer is not to be predisposed in his effort—it has been my observation that they generally proceed in a peculiar manner. From the view-point of a teacher my aims are at first well-nigh frustrated by the almost uniform insistence that the reports should be filled in with logically connected ideas *about* volition. The students rather presume that the case is an issue which must be argued out to the end in a truly syllogistic fashion, but always in the interests of some issue other than that contained in the psychological aspects of the problem. (A similar difficulty appears in the first attempts to gain clear ideas about other processes in consciousness, such as sensation, feeling, etc.) Thus, for example, it is said, "freedom is in danger," "the soul as spirit must lead to this or that view," etc. It is made out that one is to choose the starting-point for analysis solely in light of the consequences logically apprehended. I do not believe a teacher has a moment's right to allow such a notion to remain or to prevail in the minds of his pupils. It would mistake analysis of an actual process, and replace it with mere logical dialectic. Psychology must, of course, build up some arguments; but the real problem is, What shall be admitted into those arguments: syllogistic conclusions, or ideas derived from facts as original experiences?

When a tolerably full account is given of the particular percepts, images, or thought-connections, and the momentary feeling-motive which prompts the action actually performed or ideated, there is a uniform confession of 'something more there, though I cannot get at it.' Sensations, images and feelings are thus not looked upon as constituting the whole state of consciousness. Volition is set out in terms of a certain plusness, so to speak; but its factual reference must always be considered in the composition of the total state of mind. The species of particularization is horizon-filling when the enumeration of details of sensations or ideas (and possibly the same is true

of feelings) is carried out with any completeness. In the volitional reference to a conscious state, there can be an enumeration in terms of one kind only, though in the positive or negative directions. This will account in some measure for the disposition of the tendency to regard volition as 'secondary' among the phenomena of consciousness. This 'something more' is given ('felt' as often said); it is recurrent; it possesses a sequence in the specific changes in consciousness or in the physical adjustments of the organs of the body; and the descriptions of it are appreciable at least. Although every analysis makes some immediate reference to the sensory, ideational, and expressional content of consciousness, one is not thereby justified in concluding, with the current tendency, that volition is to be reduced to some one of those elements, or is statable solely in terms of those processes. The position that volition never arises as a 'pure' form of consciousness is all that is guaranteed thereby.

Another mark of the volitional process revealed by analysis is its definite and constant reference to the individual, rather than to the thing or to the 'object' as it stands represented in mind. Persons are found saying "It was, or is, or may be, my volition, or action." Sensations and ideas do not primarily present this aspect, excepting those which the individual attempts to bring into his consciousness. Movements, also, of a voluntary sort, come to be 'my,' 'his,' or 'its' movements. There is no apparent disposition to escape the individual reference when speaking of 'effort,' 'attention,' 'doing,' etc. When the object, on the contrary, by the intensity and quality of its stimulations attacks and holds me, then I am said to be giving, and describe myself as giving 'passive attention.' This is the object's attention. If *I* attack and hold the object, the individual reference returns to the foreground of the experience. Thus all attempts at an analysis of the volitional process forces a constant consideration of the personal side of the data in question as the more important and qualifying aspect. The personality thus conveyed is not the person as the content of a conception of self nor as the substance of which mind may be made.[1]

The foregoing analysis suggests the conclusion that the position which advocates volition as a mere content of consciousness, possess-

[1] A complete analysis of the process would involve a careful separation of all the typical forms of control which experience presents, such as control of retinal images, projected after-images, emotions, ideas in revery and in clear thinking, and of all succession of psychic compounds, as association, fusions, 'synopsie,' trains of images, etc., which may fittingly be reserved for another time. It can then be more clearly shown that the analysis maintained is quite in accordance with the possible development of experimental methods.

ing the varying qualities of sensations or of ideas, is not to be defended. It does maintain volition to be something actually given and ascertainable in experience, which science must recognize in an appropriate manner. The datum which must thus be recognized is a conscious relation, in which any given mental content is placed, adapting it for that expression which marks the completion of all psychological processes. The primacy of volition is not the primacy of sensation nor of feeling. Its simplicity is not such as they may have. It may itself be simple or complex, depending upon the stage and degree of development which the particular state of mind has attained. Complexity, however, is ordinarily a logical relation which is greeted and enforced as the development takes place. In fine, volition is the characteristic, typical, irreducible process into which consciousness places its definite, though varying content.

The tendency to reject the volitional process as original material for the scientific attitude has been seen to be defective when viewed in light of the epistemological considerations and of the results of analysis. There remains a third group of suggestions which have definite force against all justification of the tendency. Let us turn our attention briefly to a few principles of psychological science in so far as it should be obedient to all facts, and as those principles should be recognized in any further treatment of the process. As soon as volition is admitted as a psychological datum of scientific worth, there arises the demand that we shall seek to develop, as the first problem of the science, the inter-relations of the states of consciousness, before going to deal with the darker region of the relation of psychical events to cerebral processes. Whether we consider the mental state in its cognitive relation to its object, or as it is actually discriminable and discriminated by the individual subject of the state, the relation of state to state in consciousness is by far more clear to the student than the second type of relations. Mere mechanical sequence does not prevail exclusively among mental facts, and volition appears as part explanation of this radical difference between the psychical and the physical.

The science of psychology finds another of its special tasks to consist in an explanation of certain typical and rather uniform developments in consciousness. Mere discovery of sensational and ideational elements of experience does not replace this particular problem. There is an integration of the 'elements' of experience, which, as a fact of importance and of actuality, is not transcended by the importance of the actuality of the 'elements' themselves. How shall psychology give a lively and vital explanation or descriptive account of this inte-

gration if all recognition of volition as a datum—which shall be real in that experience and thus explanatory, rather than in need of explanation—is forced out? Integration may, indeed, be looked upon as a function, and the volitional process presents the phases which support that function. If we look upon cognition as an 'object' for the psychologist to relate to other objects (as has been set forth frequently in strongly postulated terms), then he is constrained to make explicit reference to volitional processes. Sensation as mere content and movement as mere sequence do not facilitate any explanation which is able to avoid logical absurdity. To know is to do something in a characteristic manner. And it is barely possible that the root process involved is supplied by the functional activity which volition has been seen to exhibit.

That a rejection of volition as a datum which can be appealed to for aid in really scientific explanations, virtually destroys certain principles of psychology, may be seen in any attempt to understand willing, desiring, wishing, conscious doing, etc. To explain these is a manifest duty of psychology, in so far as they are mental phenomena. That they are complex and developed does not remove the task. That they *are* such phenomena (*i. e.*, real) is attested by the experience we live, the words descriptive of these actualities which have been coined and are appropriated by all our important languages, and the reigning classifications in the science of mind itself. In order to give an explanation of will that shall be logically acceptable and patterned after the 'scientific' explanations given of ideas, or percepts, etc., one must make explicit reference to some simpler process which shall be like will in kind, but primitive in origin, and may mix with other elements of consciousness in the formation of the will compound.

Any consideration of the relation of psychology, as a science of description and explanation, to philosophical subjects shows a decided weakness lurking in the attitude which insists that volition is not to be admitted as a valid datum of the science. One cannot erect a perpendicular line of demarcation among the data of mental experience, and say that the content side belongs to psychology, while the process side belongs to philosophy. If psychology has any relation to philosophy whatsoever, then the total 'sides' of consciousness regarded scientifically must be set off to the credit of the metaphysical and epistemological considerations. To say that sensation has nothing for metaphysics because its content can be specified as visual, or auditory, or cognitive, is a defective analysis to say the least. Will and sensation are alike given in the one stream of consciousness as presented under the con-

ditions of experience. Psychology has the right to interpose an objection wherever favoritism is indulged in when assigning 'values' to the several items it may recognize. From this point of view, volition is really a datum of the world, and science and philosophy are in duty bound to turn their analytical attention to it. In other words, volition is not merely 'a point of regard' in each instance.

In conclusion, it is believed to have been shown that a dangerous tendency is at work undermining the structure of psychology. This tendency does not stand the test of the logical scheme into which scientific activity and its data should be placed. Volition, as analyzed, presents marks which entitle it to be placed among 'primitive' processes in consciousness. As all other forms of mental experience, it never stands alone, but is in constant relation from the beginning with all contents and all other functions of consciousness. The hope that all mental facts can be reduced to a few elementary and thus explanatory processes or forms, is an illusive hope, since volition cannot be 'explained' by sole reference to sensation and affective aspects thereof. Volition is ready and able to explain in a psychological manner, rather than being in need of explanation itself. To eliminate volition as a scientific datum, would be a virtual subversion of the science of psychology so long as it is developed with the intent of representing the nature of mental activity and the course of its development in terms of a real and acceptable knowledge of facts.

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VISION WITH THE MOVING EYE.

In view of Professor Dodge's experiments, published in the present number of the REVIEW, I am uncertain as to the validity of the hypothesis advanced by me to explain the lack of fusion in vision with the moving eye.[1] I had planned similar experiments, but was unable to make them, although I held my paper for a year,[2] in the hope of being able to do so. Having made the observation that there is no fusion in the vision of daily life when the eye is in constant motion, I attempted to explain the fact by assuming that the visual organs respond to retinal changes more rapidly when the eye moves than when the objects are in motion. Professor Dodge finds that there is fusion

[1] This REVIEW, 7 : 325–343, July, 1900.

[2] It was presented to the Physical Section of the American Association for the Advancement of Science in August, 1899.

when objects are exposed only when the eye is moving. It would follow from this that the hypothesis discussed in my paper, but there rejected, must be accepted or at least considered. This is, that there is clear vision only in the pauses of movement, there being no peripheral perceptions while the eye is moving and the apparent distinctness of the field being an illusion. This hypothesis will not interfere with the chief conclusion of my paper, for, as I said, "if one had in fact only a few distinct images in the course of the movement, it would remain true that a series of successive images is seen as a space continuum." Indeed, if it turns out to be true that we have no perceptions corresponding to the retinal changes while the eye is moving, but supply centrally the perception that we should have if the eye were at rest, this would further support my argument for the dependence of perceptions on utility and experience rather than on the nature of the physical stimuli.

J. McKeen Cattell.

PSYCHOLOGICAL LITERATURE.

The Psychology of Religion: An Empirical Study of the Growth of Religious Consciousness. EDWIN DILLER STARBUCK. London, Contemporary Science Series, WALTER SCOTT. 1899. Pp. 423; with Preface by WILLIAM JAMES.

Professor Starbuck's book is in substance the reproduction of two long papers published in the *American Journal of Psychology* (Vol. VIII., 1897). It is the most elaborate research yet published in a field of investigation made fertile by the use of methods which, until recently, had been unjustifiably reserved to other fields. The purpose of the book and the method employed are described in the Introduction: "It is a purely empirical study into the *line of growth in religion* in individuals, and an inquiry into the causes and conditions which determine it. It begins with conversion, since that seems to show in a condensed form some of the essential features of religious development. Then follows a discussion of the line of growth following conversion. Part II. is a presentation of the line of growth where not marked by apparently sudden changes of character. The material for the study consists largely of autobiographies written in response to a printed list of questions. The task before us is to take the varying records and find what are the common elements in them; to get a composite picture of them; to discover what are the larger aspects of religious evolution in masses of people, and to approach an insight into those laws of growth which, for the groups studied, seem natural and normal." This proposed task is so gigantic that I suspect many psychologists and the majority of the persons interested in religious studies to whose notice the book may have come have looked it over primarily to see how radically the author had failed in his foolhardy undertaking. But those who have had the patience to read him through carefully have, no doubt, closed the book, not indeed with a sense that the task had been completed for good and all, but at least with the conviction that it had been begun in good earnest and with a considerable share of success.[1] They will, moreover, have been con-

[1] In April, 1896, nearly a year before the first paper of Professor Starbuck, I published in the *American Journal of Psychology* (Vol. VII., pp. 309-385) an extensive study on conversion; it covers much of the ground traversed in the

vinced that there is here, in the patient gathering and careful consideration of the subjective facts of religious life, the foundation of a new body of knowledge which will find its place in psychological science and bear practical fruit in religious education and in theology.

That Professor Starbuck's book is open to almost numberless criticisms of detail and to a few of fundamental importance is to be expected when one considers the difficulties inherent in the subject and the too ambitious attempt made to touch upon almost every part of it. But, whatever legitimate criticism may be offered and however easy it will be for academic drones and effete theologians to hold it up to ridicule, a debt of profound gratitude is due the author for his persevering and earnest effort in gathering and interpreting a large mass of information on individual religious life. It is so much more comfortable to let loose one's constructive imagination and to spin out diverting speculations on a dubious basis of facts selected from the labors of some one else!

By far the most valuable part of the book is the first—'Conversion'—filling about half the volume. The second and the third parts —"Lines of Religious Growth not involving Conversion" and "Comparison of the Lines of Growth with and without Conversion"—offer much less interest. We shall therefore devote our analysis chiefly to the first part. Lack of space will prevent us from entering upon the discussion of the opinions and conclusions reached by the author as it would carry us beyond the space at our disposal. A discussion of several of the more important topics dealt with in this book will be found in the reviewer's paper above mentioned. In closing we shall offer a few criticism on points of general bearing.

Chap. II. The Sources.—The material on which the first part (pp. 21–183) is based consists of 192 cases of conversion, nearly all obtained in answer to a questionnaire too long to be reproduced here. They are nearly all from Protestants; the Methodists predominate. The term *Conversion* is used in a very elastic way: "The more quiet and unemotional experiences were taken along with the sudden and violent. The only test was that the person believed that the event represented a real turning-point or the beginning of a new life" (p. 24).

Chap. III. The Age of Conversion.—For the making of the

first half of the book before us. The method is the one which Professor Starbuck has used, minus the statistical tables. The third and concluding part has remained so far unfinished. To prevent misapprehensions, I desire to say that Professor Starbuck had conceived the first idea of his research and had begun to circulate one of his questionnaires before my paper appeared in print.

charts and tables of this chapter, the age of 776 converts, found in the Alumni record of the Drew Theological Seminary (Methodist), was added to the data collected by the author. The age-curves present three summits which are interpreted as follows: "We may safely lay it down as a law, then, that among the females there are two tidal waves of religious awakening at about 13 and 16, followed by a less significant period at 18; while among the males the great wave is at about 16, preceded by a wavelet at 12, and followed by a surging up at 18 or 19." The conclusions are reached that "the first two rises in the curves for conversion seem then, from the psychological standpoint, to correspond to the decline of the sensory elements in consciousness, and the birth of rational insight," and "conversion and puberty tend to supplement each other in time rather than to coincide; but they may, nevertheless, be mutually conditioned." Conversion, as the author understands it, is a distinctly adolescent phenomenon and is connected with the physiological growth of that period of life.

Chap. IV. The Motives and Forces leading to Conversion.—An insignificant chapter on a most important subject; eight pages with two statistical tables showing one 'the relative frequency of certain motives and force which lead to conversion,' the other 'a comparison of the revival and non-revival cases in regard to the motives and forces leading to conversion,' and curves 'showing the frequency of various motives for different years.' We reproduce the first table (p. 52).

Motives and forces present at conversion.	Females.	Males.	Both females and males.
	Per cent.	Per cent.	Per cent.
1. Fear of death or hell	14	14	14
2. Other self-regarding motives	5	7	6
3. Altruistic motives	6	4	5
4. Following out a moral ideal	15	20	17
5. Remorse, conviction for sin, etc.,	15	18	16
6. Response to teaching	11	8	10
7. Example, imitation, etc.,	14	12	13
8. Social pressure, urging, etc.,	20	17	19
Sum of 1 and 2—*Self-regarding* motives	19	21	20
Sum of 3 and 4—Other-regarding and ideal motives	21	24	22
Sum of 1 to 5—Subjective forces	55	63	58
Sum of 6 to 8—Objective forces	45	37	42

From this table the two following propositions are derived, rational considerations play a small part in conversion; "males are controlled more from within, while the females are controlled more from without."

The three following chapters (Experiences preceding conversion; The mental and bodily affections immediately accompanying conversion; In what conversion consists), always in statistical form, prepare the way for Chap. VIII., the pivotal chapter of the first part. Here is one of the several tables found in these chapters; it shows 'the relative prominence of the various mental and bodily affections for both sexes.'

Mental and bodily affections.	Female.	Male.	Total.
Sense of sin	33	33	33
Feeling of estrangement from God	24	24	24
Desire for better life	18	18	18
Depression, sadness, pensiveness	75	60	70
Restlessness, anxiety, uncertainty	45	33	41
Helplessness, humility	12	9	11
Earnestness, seriousness	12	1	8
Prayer, calling on God	39	57	45
Tendency to resist conviction	36	50	41
Doubts, questionings	6	15	9
Loss of sleep or appetite	26	29	27
Nervousness	6	5	6
Weeping	9	3	7
Affection of sight	5	6	5
Affection of hearing	3	7	4
Affection of touch	14	12	13
Average duration of conviction	24 weeks.	69 weeks.	

Here again, according to the author, we see that "conversion is a process in which the deeper instinctive life most strongly functions." This is no doubt true, but I am at a loss to see that these statistics bear that interpretation. Much space and figuring are devoted in these three chapters, and also throughout the book, to a comparison of the sexes with each other, and of conversion as it takes place in revival meetings and out of it. Here are samples of the propositions drawn from these comparisons: "feeling plays a larger part in the religious life of females, while males are controlled more by intellection and volition;" "the males when not at revival insist more upon seeing their way clearly toward the new life, they are more wilful in the stress of a revival, and the revival conversion is a far more intense experience."

"The sense of sin and the accompanying depression of feeling are fundamental factors in conversion if not in religious experience in general. We must not look upon the sense of sin as simply a spiritual fact, but we have to look for the cause underlying the sense of sin, in part, in certain temperamental and organic conditions." (Pp. 67 and 71.) As a conclusion of the preceding analysis the typical conversion

experience is described in Chap. VI. in a few barren words, "The typical experience has three distinct features, viz., dejection and sadness, a point of transition, and lastly, joy and peace." At this point the reader should have before him a clear mental picture of the conversion experience, but it can hardly be so as not one whole case has been given him; instead isolated sentences from the respondents, now on 'sin,' now on 'estrangement with God,' now on 'love for others' or some other part of the experience fill the volume. It is as if instead of seeing a few typical heads one was shown successively dozens of noses, of upper lips, of under lips, of chins, etc. What is the meaning of an eye seen apart from the rest of the face? The reader, unless he be intimately familiar with the experience dissected, will have the impression of moving in a maze of disjointed facts and much of the meaning of the book will escape him.

Professor Starbuck is aware of the large place that must be accorded to unconscious cerebration in the dynamics of mental life in general and particularly of conversion, and he has noticed that self-surrender, self-renunciation, is very frequently an apparently necessary condition of it. Hence, when he comes in Chap. VII. and VIII. to close quarters with the causal determination of the conversion crisis, he finds it necessary to consider at some length the rôle of the will and of consciousness. In Chap. VIII. the problem is formulated thus: "The question is not simply how much of conversion is willed, but how much of the process as it is being wrought out rises into consciousness; and on the other hand, is there evidence that part of the process is worked out automatically by the nervous system?" Arguments and facts have already been given to show that a part of the phenomena preceding conversion are instinctive. It should not be thought that when at the decisive moment of conversion the will of the individual is passive it is therefore not concerned in the process at all. On the contrary, the final victory, the new birth, is in a large measure the result of previous acts of the will striving towards righteousness. (Pp. 97–100.) How this is possible the author endeavors to make clear to the lay reader by a digression on the effects of past unconscious upon present conscious activity, and of past conscious upon present automatic activity. In the three closing pages of Chap. VIII. an effort is made to grapple with some of the most intensely interesting and fundamental psychological questions raised by the conversion experience as the passivity of the will, faith, and the nature of the new life. I can do no more than quote the italicized passages: "Self-surrender, then, is often necessary in order

that the normal tendencies of growth may converge and flow into harmony, and that the point of new insight may be, for the person yielding, the truest organizing center of life." "The act of yielding, in this point of view, is giving oneself over to the new life, making it the centre of a new personality, and living, from within, the truth of it which had before been viewed objectively."

Chap. IX. The Quality of Feeling following Conversion.—Joy and peace now follow the anxiety and despondency of the pre-conversion period. A common experience with new converts is the *sense of newness*. Two statistical tables are given. The author looks upon these feelings as the token of 'the functioning of a new and exalted personality.' This view of conversion is developed in Chap. X. and particularly in Chap. XII. At conversion, life is organized around a new center, around impulses which may be characterized roughly as altruistic—it is an 'unselfing'—to contrast them with the egoistic motives forming the center of the old personality, hence the larger life of the subject and the birth of new powers. In Chap. XII. ('A general view of conversion') the new life is looked upon from the physiological and from the psychological point of view as the formation of a new person. Reference is made to the growth of the brain fibers, their medulation, association, etc., and thus some plausibility and definiteness are given to neural changes supposed to take place simultaneously with the formation of the new *ego*.

Some valuable light is thrown in this chapter upon the problem in discussion.

Chap. XI. ('Conversion as a normal human experience') gives many instructive instances of depression and joy, of sudden awakenings, of sudden changes in emotional attitude and of breaking of habits, occurring independently of religious influences. These illustrations were obtained in answer to a special set of questions; they are given to add weight to the presumption that conversion can all be explained in terms of natural psycho-physiological processes.

A chapter on the abnormal aspect of conversion concludes the first part of the book.

The data used in the second part (pp. 183–350) consist chiefly of 237 answers to questions on religious growth designed for persons who are strangers to the conversion—experience. It includes chapters on the religion of childhood, on various phases of adolescence (six chapters; religious awakening, storm and stress, alienation, etc.), on the period of reconstruction following adolescence and finally three chapters on adult life. The chapters on adolescence deserve especial attention.

The third part (pp. 353–420) is an attempt to trace out the chief lines in the growth of religion from childhood to maturity and to compare growth via conversion with growth not including it.

I have, I trust, sufficiently expressed my appreciation of the solid worth of Professor Starbuck's work to venture the following general criticism without running the risk of unduly depreciating it in the eyes of the readers of this REVIEW.

I have already deplored the absence of complete religious biographies. At least one of each of the several varieties of experiences gathered by the author under the name conversion should be found entire in the book.

One of the serious weaknesses of the book is the totally insufficient use made of introspective analysis. To yield the best result, introspective analysis, and the kind of interpretation and of explanation which it makes possible, should have gone side by side with the objective consideration and the statistical tabulation of the data. But the author has chosen to remain outside his facts, as if he was handling physical substances. Throughout the volume we deal—we might say *figure*—with *things* called sense of sin, remorse, example, response to teaching, depression, feeling of estrangement, tendency to resist conviction, sense of oneness, divine aid, etc., etc., and no effort is made to analyze these things of which the respondents speak and to translate them in psychological language. The *words* in circulation in religious circles are too often the coins with which the business of this book is transacted. Phrases like the following make one balk when met in the writings of a professional psychologist: 'A vital experience of spiritual truth,' 'the attainment of spiritual life,' 'the person has acquired a spiritual grasp, a new insight,' 'the higher life of intelligence and insight,' 'a personal hold on virtue,' 'a first hand perception of right and wrong.' An investigation exclusively from the objective standpoint, the only one available to the physical scientist, is self-evidently doomed to remain on a very low explanatory level when its objects are processes of immediate experience.

What shall be said of the statistical method which gives to this book a unique appearance of scientific rigor? The propriety and the advantage of using statistically the material obtained by means of syllabi on subjects as complex as religious life is open to weighty objections, the chief of which arise, in the case before us, from the loose meaning ascribed to the term conversion, from the unavoidably confused and inexact language of the correspondents describing their experiences and from the comparatively small number of answers tabulated.

It is evident from the citations scattered through the book that a definition of conversion, as the one we have quoted at the beginning of this review, tends to bring together experiences differing greatly from each other in quality as well as in intensity. In some instances a mature person relates an experience covering years of moral struggle and reaching down to the springs of life; it culminates in a crisis from which the subject issues a new man. In other cases what is described are the skin-deep, momentary, feelings of a sentimental adolescent female who, in the excitement of a revival meeting, goes to the altar 'to give her heart to Christ'—an experience which hardly leaves any trace behind. The study and the mutual comparison of these and other 'conversion' experiences is evidently a legitimate task, but to put them together in order to treat them statistically comes dangerously near figuring without paying attention to the place of the digits. The author speaks of obtaining in this way a 'composite picture.' What kind of composite picture would be obtained from faces in which the distance between the eyes varied from 1 to 2 inches? An elongated grayish spot twice the length of an average eye and increasing in darkness toward the center would be the composite eye; a most instructive blur! We cannot enter into a detailed examination of the effect of this procedure; the whole book is more or less vitiated by it; we must be content with an illustration involving the chapter on the age of conversion. The average age of conversion for male is given as 16.4 years and that of its most frequent occurrence as 17. The average age of the conversions I have gathered for my printed paper is around 25 and the year of its greatest frequency is not far from the same figure, this simply because my questionnaire was understood to call for descriptions of conversions of the deep, life-transforming variety; the variety that can occur only when the great plasticity of early youth has been superseded by a character more firmly knit. What becomes then of the ingenious and interesting generalizations of Professor Starbuck putting in relation the age of conversion with that of puberty and of greatest bodily growth? They hold exclusively for one variety of conversion, a variety to which a large number of the conclusions reached in the subsequent chapters do not apply. The conditions imposed by the statistical method have not been fulfilled in this book, therefore the statistics, as far as they lump together experiences which should have been held separate, produce but a spurious precision and often introduce confusion and error instead of distinctness and light.

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The Making of Character—Some educational aspects of ethics. By JOHN MACCUNN, M.A., LL.D., Balliol College, Oxford. New York, The Macmillan Co. 1900.

The person who takes a well-worn and hackneyed subject and by a fresh and vigorous treatment imparts to it a new and facinating interest, has performed a real service for his kind. The more fundamental the subject the greater the service.

Innumerable books have been written on 'character.' The subject has been talked about and preached about from the beginning to the present time, and it would seem as though the last word had been spoken, but here we have an author who ventures to risk his reputation as teacher and scholar in writing a book on this venerable topic. That he has abundantly succeeded, no one will doubt who, possessing a liking for ethical studies, will peruse the work.

The point of view is that of the educator, and the current dictum that the highest aim in education is the formation of character, is not overlooked for an instant. The table of contents indicates that all the forces, natural and artificial, which operate to produce the man or the woman, are taken into account. Under the head of 'Congenital Endowment,' Heredity, Vital Energy, Temperament, Instincts, Development and Repression, Habit and its Limitations, are all carefully considered.

The author never fails to seek a just balance between what is natural and what is artificial in life. He says: "It is an impossible task to discriminate sharply between what is congenital and what is due to the influence of environment. Environment begins to operate with the beginnings of life, nor does it ever cease to operate, not for an instant, as the days become months and the months years."

His view of what temperament is reflects the same broad and comprehensive method. "Of this mixing of the elements, temperament is the reflection inwrought in the very texture of the life that modifies all we receive and from first to last conditions all we do. It is, so to say, the medium that colors and suffuses all experience."

Under the general caption of 'Educative Influences' the book deals fully and specifically with bodily health, reactions, family, school, friendship, livelihood, citizenship, and other social influences.

Professor MacCunn finds a close relationship between the physical and the moral. "Idleness, says proverbial wisdom, comes to want, but its worst want is not the empty purse; it is the soul atrophied for lack of the spiritual wages that never fail the strenuous life. What holds of idleness holds likewise of languor and weakness."

The chapter relating to the Wordsworthian education of nature is particularly suggestive. "Exclusive preoccupation with society is not the way to know it best. Through communion with nature man may come to understand life better."

While great stress is laid upon the physical basis of life and growth, the author does not overlook the importance of moral ideals and especially of those ideals which dominate social activity. He finds that not only in the personal life but in all organized effort there are guiding principles which inspire the human heart. "It is precisely in the sphere of industrial and commercial life that ideals are most needed to uplift the practice of the world."

This profound study of social and ethical values finds its climax in the two concluding chapters on Self-Development and Self-Control. Here we have the latest and best educational thought of our time. No word has been spoken which better voices the deep convictions of parents and teachers to-day than the idea that all growth and training must seek to develop a self-poised, self-governing being who finds his freedom in the proper use of all his powers and who experiences his highest joy in serving his fellow-men.

SAMUEL T. DUTTON.

COLOR VISION.

Ueber relativen absoluten Mangel des Farbensinnes. E. RAEHLMANN. Ztch. für Angenheilkunde, II., 315, 403. 1899.

Raehlmann adds some observations on partial color-blindness, but by far the most important part of his two articles is that which gives a detailed account of a very remarkable case of total color-blindness, the only one hitherto discovered in which the eyes of the patient were in every other respect perfectly normal. No trace of disease or of abnormity could be detected except the lack of color-vision: there was perfectly normal visual acuity and rather more than that, normal 'light-sense,' and normal visual field. The distribution of brightness in the spectrum, however, resembled that of no other case of either disease or health; there was an extreme darkening in the part which is yellow to normal eyes, so much so as to divide the spectrum almost into two separate regions; the normally blue part, on the other hand, was distinctly intensified. Raehlmann did not apparently make use of a spectrophotometer, but there seems to be no doubt that the patient was extremely insensitive to, Raehlmann says, yellow light—it would be better to say light of the yellow wave-length, as yellow

light did not exist for her. Here as in other respects, language needs some remodeling in the interest of more accurate distinction between the objective and the subjective. The patient, an intelligent lady and the wife of a university professor, had unusual powers of discrimination in regard to her sensations, and unusually good capacity for observation. There are several cases of color-disturbance in her family, and especially that of a son of the Frau Professor herself; his vision is dichromatic, but he has very keen sense of difference of brightness. Mrs. R. is wholly lacking in any idea of what a color is. It is said that an oil-painting does not correctly represent to her the relative brightnesses of a landscape, which are better given in a photograph, and that a picture gallery seems to her an absurdity. The spectrum is increased in length at both ends, more especially at the violet end; it is brightest in the green—there is also a maximum in blue and again in red. Adaptation to a faint light was normal. The threshold in the yellow part of the spectrum was extremely high, but in the blue it was far lower than for the normal eye. Visual acuity, which was normal at an ordinary illumination, was in an extremely faint light twice as good as that of any of the five assistants of Professor Raehlmann who examined her. After-images were normal, but simultaneous light induction seemed not to take place (p. 331). There was astonishingly acute perception of brightness differences, even at a strong illumination, and there was no trace of photophobia.

This case of Mrs. R., while it is absolutely unique, attaches itself readily to the cases of dichromasy with perfectly undisturbed vision in other respects, which are no rarity. It is evidently to be referred to some defect of development in the visual centers, and is to be connected with those cases of achromasy in which (although vision is markedly defective) there is no foveal blindness, and hence there is no reason to believe that the entire cone-system is out of function. That the brightness curve is totally different from that of nearly all the other achromates and also, consequently, from that of the normal eye in the reinforced vision of darkness, is by far the most remarkable phenomenon presented by the case, and it must stand for the present as something totally disconnected with all the other facts of vision. So far as it goes, it gives substantiality to the idea of an abnormal shift of excitability (not, indeed, in the retinal visual substance but in the cortical receiving stations) of which the Helmholtz school makes so much in its explanations of defects in color vision. But even before this the shift of excitability hypothesis could not be said to be an instance of *non vera causa*, for already there were known the cases of what are

called anomalous color vision in which the maximum red sensation is produced by a light wave of not the same frequency as in the normal eye. These cases, too, it is not the custom to keep sufficiently in mind.

Raehlmann speaks of this insensitiveness of the eyes of Mrs. R. in the region of the sodium line as if it were a sort of yellow blindness over and above the absence of specific color sensation throughout the spectrum; and he even says (p. 334) that in a sense every gray-sensation contains a color component (because it is produced by ether rays, which, if they acted alone, would give rise to a color-sensation). But this is visionary in the extreme.[1] It is the great advance of recent years in this field to have shown that the quenching of opposite colors is an affair of physiology quite below the level of consciousness and that the colors which are mutually destroyed have never had an existence as colors. Here again appears the need of a better phraseology —of distinguishing between the spectral *colors* and the regions in the gamut of ether-vibrations which give rise to them.

This case ought also to have the effect of causing us to accentuate more than we do now the difference in nervous level at which color-defect may take place. The central color scotoma of the tobacco and alcohol amblyopia is surely due to disease of particular bundles of the optic nerve trunk—a sort of fact that the psychologist is too little apt to have in mind; in general not enough attention has been given, in the discussion of color-blindness especially, to such pathological cases as are plainly due to retrobulbar lesions—there has been too constant an attempt to explain everything by abnormal processes in the retina. On the other hand, the authenticated cases of uniocular color-blindness can be explained only by an ante-commissural defect; it is, therefore, necessary to bear well in mind that different cases of defective color-sense may be of widely different origin.

Raehlmann calls attention to a very important experiment made by himself twenty-five years ago (Arch. f. Ophth., XIX.) in which a dichromate was given santonine with the effect that his yellow-blue vision was reduced to vision of a single quality only, blue; the whole warm end of the spectrum became to him gray. Such a case as this speaks strongly (and there are other indications leading in the same direction—cortical lesions, chromatopsias, etc.) in favor of their being (whatever the number of chemical processes in the retina) four distinct centers in the cortex for the four fundamental hues. Knies

[1] Moreover, he quite forgets that in night-vision, and in the achromatic vision of the extreme periphery of the eye, there is no question of color-components (in the subjective sense) being by any possibility present.

seems to have shown (Arch. f. Augenh., 37, p. 102) that in santonine color effects the source of the change is not any coloring of the retinal layers of the eye—that is, he draws this conclusion from the fact that in dogs and frogs, after very strong doses of santonine, the visual red, after darkness-exposure, had its color not in the least disguised.

C. LADD FRANKLIN.

The Order of Development of Color Perception and of Color Preference in the Child. By WARD A. HOLDEN, M.D., and K. K. BOSSE, B.S., New York City. Archives of Ophthalmology, Vol. XXIX., No. 3. 1900.

In this paper are described the results of a series of experiments made on over 200 children between the ages of six months and thirteen years, relative to some of the phases of developing color perception. In the first part of the paper, by a method devised by Dr. Holden, the child is tested for its absolute color perception by making use of a background shaded from dark to light gray upon which squares of colored tissue paper are so placed that their luminosity is equal to that of the gray background. The child that reaches for a colored square under these conditions, must perceive that color as differing from gray. At the age of six or seven months infants react readily to red, orange and yellow but without much certainty to green, blue and violet. At eight or nine months they react promptly to the first three, and sluggishly to the last three. At ten or twelve months there is often equally prompt reaction to all colors.

Preference tests carried out with satin ribbons showed that children below three years preferred the red end of the spectrum, selecting red first and then following very closely the order of the spectrum, and selecting blue or violet last. After the end of the third year the order of selection is reversed, however, blue or violet being chosen first and red last. This remarkable change is accompanied by a general disinclination on the part of the child to make any selection at all, so that at the age of about three years only a small percentage could be induced to perform the required action. The results thus obtained were tabulated and the figures give a fair picture of the gradually changing conditions. As a brief summary: The child passes through these phases of development: Up to about the third year the red end of the spectrum is preferred, then follows a period in which the attractiveness of the blue end increases until it becomes as strong as that of the red end, the consequence being a disinclination to make any

selection at all. This period may vary greatly in duration. Later the attractiveness of the blue end increases with the result described. The table in the original shows also that, while at the beginning of the preference for blue the reverse order of the spectrum is followed closely, with advancing age this order changes gradually and approaches the order of preference found in adults. Thus the young infant reacts in a physiological way to the more exciting colors of the red end of the spectrum. The older, mentally advanced child rejects the exciting reds and oranges and prefers the more restful colors of the blue end of the spectrum. These results may lead to further investigations. The described changes of preference in a child of about three years must certainly be parallel to alterations in the mental make-up. If we consider that at this age the reflective stage has begun to take the place of the earlier impulsive stage, the results should yield valuable data as to the character of some of the mental changes in the child. It is likely that an investigation into the development of the other senses would give results analogous to these.

K. K. Bosse.

MEMORY.

Experimentelle Beiträge zur Lehre vom ökonomischen Lernen. Lottie Steffens. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Band 22, Heft 5. Pp. 321–382.

This paper is chiefly devoted to a report of a series of experiments dealing with the methods by which various persons memorize verse. Having discovered a number of such methods, the author undertakes to determine which form of procedure requires the least expenditure of time in order to learn by heart any given selection of poetry. Throughout the article the term economy applies to the amount of time, and not to the amount of effort, involved in any of the various undertakings. The larger part of the experiments were made upon seven subjects. This is relatively a small number, but the results are sufficiently uniform to warrant considerable confidence that they represent typical tendencies at least. At a number of points the evidence was supplemented and confirmed by experiments with nonsense syllables.

Apparently the natural disposition of most persons, when attempting to memorize verse, is to split the selection up into a number of sections, which are learned more or less independently, and then put together. This general method shows many individual modifications, both as regards the length of such sections, and as regards the relative

number of repetitions accorded the several parts after they have been once learned. Very rarely is there any tendency manifested to learn an extended piece of verse as a whole, *i. e.*, to read it entirely through without the repetition of words, lines or sections. Unusual as this procedure seems to be under ordinary conditions, the experiments show uniformly better results when this method is adopted.

The author advances a number of plausible explanations for the common prejudices in favor of the piecemeal mode of memorizing and then attempts to prove in what particulars the other method is superior, calling to her assistance for this purpose nonsense syllables. The method of learning the selection as a whole has the obvious advantage of emphasizing the really helpful links of connection between the various parts, instead of the fictitious and misleading ones, which often arise when the material is learned in sections. Involved in this is the helpful influence of associations due to the absolute position of given elements in the whole. The importance of this factor will be readily understood by those who have experimented at all extensively with the memory processes. On the other hand, it is to be said that when the material to be memorized is not reasonably homogeneous, the segmental method may be more advantageous. The reason for this is obvious. A peculiarly baffling group of words may render many repetitions necessary, and where the whole selection is read through consecutively, the loss of time is inevitable. With homogeneous material, however, the advantage of this method is roughly proportional to the length of the selection. There is seemingly little tendency, when this method is employed, to rely upon purely mechanical motor processes, by means of which children often commit quantities of poetry to memory without the slightest suspicion of the meaning. Clearly the value of logical significance is at a maximum with the method. It promises, with material of uniform difficulty, the least amount of unnecessary and superfluous repetition of any portion.

Incidentally, in the course of the paper, a number of interesting points are touched upon, such as the relation of rapidity of reading to rapidity of learning; the dominance of sensory or motor elements in the different modes of memorizing, etc.

A final section discusses a series of experiments dealing with the question of the most advantageous method of distributing any given number of repetitions throughout a constant time. Here again nonsense syllables furnish the material. The general results indicate that the distribution of repetitions which affords the best results is that which secures the maximum number of *groups* of repetitions, with

intervals of time between the groups, and the minimum number of repetitions in each group. This means doing a little at a time and doing it very often. Like the conclusions of the first part of the paper, this runs counter to common prejudice and practice.

The author makes no attempt to discuss the question of permanence of retention, as connected with her problem of expeditiousness in learning, nor is there any notice of the modifications in her results, possibly due to the metrical nature of her material. This is not to be interpreted as a criticism of the work, which seems to have been intelligently planned and carefully executed, but simply as an indication of its limitations.

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PERCEPTION AND RETENTION.

Zur Untersuchung der Auffassungsfähigkeit und Merkfähigkeit. JACOPO FINZI. Psychologische Arbeiten (Kraepelin). Bd. III., 2 Heft, 1900. Pp. 289–384.

Finzi's investigation is a methodological inquiry in which he undertakes a more exact examination than has heretofore been made of the disturbances of memory in the normal individual, for the purpose of applying the method in the diagnosis of mental diseases. It is impossible to determine the quantity of impressions which his past life has furnished to the patient, but it may be useful for the alienist to ascertain his capacity for receiving and retaining impressions at present.

The writer seeks to measure two things: the clearness of the impression and its fixity. In connection with the latter aspect we must separate the effects of fixation through voluntary repetition, and the involuntary retention of impressions which follow directly from their presentation. For his material the writer turns to visual impressions, and adopts the customary letters, syllables and pictures. The mechanical conditions of experimentation seem to have been carefully looked after and the apparatus for control of the exposure is fully described and pictured. To secure constancy a Welsbach lamp was used as the source of illumination and the figures were seen by transmitted light. The time of exposure for each point of the surface was approximately the threshold for the perception of letters, viz., 16.7σ. The action of the apparatus was under control of the observer himself. Five sets of experiments were carried out. In the first the interval between presentation and recall varied, the periods being 2, 4, 8, 15 and 30 secs. In the second a constant interval was filled by such distracting media

as counting, adding and reading. In the third the number of stimuli was varied, 4, 6 and 9 letters being used. In the fourth the stimulus was reinforced in various ways—such as reading aloud or writing down—or was not expressly reinforced in any way. In the last set different kinds of stimuli were obtained by the use of letters, numbers and syllables. Only the first of this series, it must be noted, was carried out on a sufficient number of persons to give a basis for more than individual results.

Two factors which appear in the results are discriminated in the writer's analysis. These are the total number of elements perceived or retained, and the number correctly apprehended or recalled. The former is taken as an index of the scope, the latter of the reliability, of the process involved.

The character of the results depends upon a group of conditions which includes both the properties of the material and the mental activities of the subject himself. Among the former are to be reckoned the comparative legibility of the letters and figures, the number of them and of their possible combinations, habitual verbal association, and the like. On the part of the subject the prevalent concentration of attention upon the first cross row of elements—which were presented in three horizontal rows of three—led to the fixation of particular groups of letters and the overlooking of others; the images of one card's contents were in many cases influenced by confusion with letters from preceding cards; and various subjects showed preference for particular letters.

The increase in the number of elements presented has an unfavorable effect upon the scope of apprehension, marked by increase in the number of misplacements; for the retention of the impression it has little significance.

In neither scope nor reliability is the maximum effectiveness of the perception process exhibited immediately after presentation. The former phase attains its highest value between eight and thirty seconds after exposure, the latter at an earlier period, between four and fifteen seconds. Thereafter in both directions a falling off appears. The increase of error may continue, as is to be expected, for a much longer period.

The prevailing forms of error are misplacements, substitution of resembling letters, and those arising from preference for particular letters and the influence of preceding letter-groups. Numbers are better apprehended than letters, but not so well retained. Perception is aided by the fewness in number of the digits as compared with that

of letters, while the greater retentiveness for letters is probably to be referred to verbal associations and grouping. This factor appears again in the statement that letters are better apprehended and retained when presented as parts of nonsense syllables than when seen singly.

The mechanism of retention appeared to affect the process of revival, dependence upon images of the material presented leading to better results than such as involve the use of images of speech.

The effect of distraction during the interval between presentation and recall appears in a reduction of the correct responses while the number of incorrect reports may be either increased or diminished. The latter must be regarded as an individual variable, since while in one subject the fading out of the impression would be marked by an increase in the number of incorrect reports, the total number of impressions reported remaining unaffected, in one of less confident nature those cases upon which uncertainty lay would not be reported at all. As the intervening interval increases subjective certainty declines with greater rapidity than the number of correct responses. Practice increases the reliability of the perception or revival process to a greater degree than its scope. The results of the investigations present little that is new in direction, and such elaborately detailed experiments are justified only in so far as their carefully guarded conditions constitute a needful exactitude in the corroboration which they give to the work of previous investigators.

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REACTION-TIME.

Reactionzeiten bei Durchgangsbeobachtungen. NICOLAUS ALECHSIEFF. Philosophische Studien, XVI., Heft I., pp. 1–60, with two charts. May, 1900.

The paper gives a brief history of the personal equation, an analysis of the different types of reaction, and a discussion of the conditions most favorable for transit observations.

Many of the causes of the differences in these observations, first noted by Maskelyne in 1795 and studied later by Bessel (1822), have since been made the subject of experiment and analysis at different observatories. With the old eye-and-ear method the differences were not the same between different observers; in some cases they remained small and constant, in others they changed progressively (Greenwich) or even suddenly—a difference of .22 sec. developing in one night at Altona. Respold (1838) and Arago (1842) introduced the chrono-

graphic method. With this the differences became smaller and more constant. Prazmowski and Hartmann studied the influence of practice and imagined the error of observation might be reduced to zero by preliminary work with a machine that would show the extent of error made. By means of an artificial transit experiment Hirsch and Plantamour found that the time between the perception and the registration varied from .15 to .36 sec. By others, the influence of the size, intensity, and direction of motion of the star, and the position and exhaustion of the observer had been noted; but a number of psychological factors have not been taken into account by astronomers, particularly the effect of directing the attention on the one hand to the star and on the other to the registration, a variation not known even to psychologists till the publication of Lange's paper in the *Studien* in 1888. This gives us the two-fold purpose of the investigation—the analysis of the different types of reaction, both in the usual form of experiment and in transit observations, and secondly thereby the ascertainment of the best conditions, particularly psychological conditions, for such astronomical work.

In the tabulation of results the author makes use of the well-known statistical method of the frequency of cases. The number of reactions falling between successive hundredths of a second are counted and the results, which appear in both tabular and graphic form, give a better picture of the characteristic variations than could be obtained by the customary average and mean variation. In fact the records in a series by the same person are sometimes so distributed that the arithmetical average falls between two points of high frequency rather than at the point of greatest frequency. This is particularly so in the 'natural' reactions in transit observations—only artificial transit observations are made throughout in which a tendency to employ now a long and now a short form of reaction in the same series was observable.

The sensory and motor or, as the author very properly prefers to call them, the more and less complete, or the longer and shorter, forms of reaction are to be looked upon as extreme variations from the 'natural' type, or that which the reactor adopts spontaneously and in which his attention is directed according to need, both to the receiving of the stimulus and the making of the reaction. Considerable practice is necessary to develop the extreme forms, and there is at first a tendency to revert to the 'natural' or intermediate type. The short extreme is developed by slurring the sensory part more and more; the long, on the contrary, by directing attention to it. In both

of these modes of reaction in which the attention is directed by all uniformly to a certain part of the process, the differences between the subjects tend to disappear. In the intermediate form, however, these differences remain prominent, though to a lessening degree with practice; and they are attributed to constant differences in the direction of the attention.

The author's mode of tabulation, and especially his transit experiments with the 'natural' mode of reaction, enable him to give an excellent account of the production of these longer and shorter forms of reaction. But while it is rather easy to see how the subjects may pass by training from the 'natural' mode of reaction, on the one hand, to the quicker and more nearly automatic; and, on the other, to the more cautious and hesitating type, in which a somewhat distinct interval is recognized between the perception of the stimulus and the making of the reacting movement, and how various conditions might lead the reactor to pass from one to the other, or to stop at different stages, some of which would naturally come to be preferred by practice—several peaks are found in the frequency curves of the long reactions—it may be questioned whether the author is right in assuming, with others of the Wundtian school, that turning the attention to the reception of the stimulus will always result in the longer forms of reaction, or that sensory reactions and these longer forms are one and the same thing. They may very well be separable, and the direction of the attention to the stimulus may not be the essential thing in the case. The evidence offered by American observers in which the sensory reactions are as short or shorter than the motor, would give this hypothesis a certain degree of probability. Even that the longer reactions are to a proportional extent more complete, may be doubted and should not be assumed without analysis.

The chief causes of the large individual differences found in transit observations are lack of practice, the use of different modes of reaction, and the following of the star with the eye instead of fixating the cross-lines. The shorter and so the more uniform mode of reaction is the better for astronomical work, though an extremely short form cannot be attained, because the stimulus demands some attention. The 'natural' mode of reaction, however, permits the analysis of the psychological conditions the better and so was generally employed. The long form cannot be used as the moving star almost unavoidably prepares the mind for the shorter reactions. The movement of the star must, however, be of a certain rate for this to be true, namely, a movement between 10° and 14° per second. Lower speed occasions long re-

actions and quicker marked irregularities—the extent of the field of view remaining the same, namely, about 28°. As the entrance of the star into the indirect field of vision served as a warning signal, this makes the favorable preparation interval from 1 to 1.5 seconds. To a certain extent, the smaller the field of view with a rate of 14° per second, the quicker and more uniform the reactions even when the preparation time was reduced considerably below a second. The direction of motion of the star makes little difference if the cross-lines are fixated in the observations, but it is otherwise when the eye follows the star since a movement from left to right as compared with a movement from right to left causes a lengthening of the records, if the right eye is employed.

Astronomers use eyepieces with several cross-lines, and other stars beside the one observed may be seen moving across the field of view. When these conditions were reproduced artificially, it was found that the record became shorter and more uniform as a result of the intensifying effect of the rhythm of observation and movement developed.

As in this instance, the successful application of psychology to a practical problem may have to wait long for fundamental discoveries to be made, but when such application is made it certainly deserves a special welcome.

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PATHOLOGICAL.

De l'action de la fatigue sur la structure des cellules nerveuses de l'écorce. G. GUERRINI. Archives italiennes de Biologie, XXXII., Fasc. I. Pp. 62–64.

After a brief review of the results in this direction obtained by Tissié, Hodge, etc., the present investigator in this 'preliminary note' describes his own work, emphasizing especially that therein for the first time the stimuli to fatigue were purely physiological in their nature, and that their duration and the intensity of the resulting fatigue were both exactly measured. Twelve dogs were induced to run distances varying between 35 km. and 98 km., until obviously fatigued. The animals were then killed, and, under special precautions, their cortical cells, both cerebral and cerebellar, were examined. The alterations found were 'both numerous and interesting,' and may be summarized as follows:

"1. Augmentation of the pericellular lymphatic spaces. 2. The presence of leucocytes on the cell in the pericellular space. 3. Disorder of

the achromatic network. 4. Decomposition of the chromatic plaques, which were sometimes reduced to mere bits. 5. The presence of vacuoles in the protoplasm. 6. An irregular appearance of the contour of the nucleus. 7. The presence of vacuoles in the nucleus." These the researcher considers to be proportional to the fatigue displayed by the animal rather than to the distance he had run. They are more evident in the cerebral cortex than in the cerebellar cortex, and are always more distinct about the 'motor zones.'

Sur le mode de so comporter des différentes sensibilités sous l'action de divers agents. Drs. G. Boeri and R. Di Silvestro, Naples. Archives italiennnes de Biologie, Vol. XXXI. Pp. 460–464.

This article is its authors' summary of the results they obtained from a study of the different dermal sensations and of the muscular sense under various pathological conditions artificially produced for the purpose, with especial reference to their respective degrees of persistency. As subjects they employed themselves chiefly, but also some women far advanced in pregnancy and others with cirrhotic, much enlarged livers. For instruments they employed, in studying touch, Weber's compass; pressure, Eulemburg's baresthesiometer; pain, the faradic apparatus of Lewandowski; in studying thermal sensibility, a thermesthesiometer invented by one of themselves; and for the muscular-sense investigation, they used a wire skeleton covered with cloth on which weights were placed. In each case they first studied the individual in the normal condition.

The effect of *distention* of the skin was studied on the abdomens of the women subjects. *Compression* of a nerve was produced, in the case of the ulna, by digital compression, and in the sciatic by resting a long time on a horizontal bar. *Refrigeration* was produced by placing the member to be used in melting snow, with or without salt. Artificial *anæmia* was brought about by application of the expulsive bandage of Esmarch. *Hyperæmia* was somewhat similarly produced by mere constriction until the limb was purple.

Their results may perhaps be best given in their own terms: "We found sensibility to *pain* to be the most resistant of all to the agents to which they were submitted, save that to cold it was the least resistant of all. *Thermic* sensibility is, next to pain, the most resistant, with the same exception as regards refrigeration, where the contrary is the case (thermic sensibility is more resistant to warmth, or, stated better, by our mode of research, augmentations of heat are better determined than lowerings of temperature). Sensibility to *pressure* acts

somewhat as does the tactile sense in that one cannot always say which of the two is the least persistent; both, and perhaps especially sensibility to pressure are little resistant, except to refrigeration, to which the latter is very resistant. The *muscular* sense also preserves itself well in cold, diminishes a little with compression, and disappears with anæmia produced à la Esmarch."

These sensibilities have, the authors think, discriminating physiological differences which make them properly considered as independent of each other, for in unnatural conditions like these they vary differently, one or the other diminishing more than the others.

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NEW BOOKS.

La Philosophie d'Auguste Comte. L. LÉVY-BRUHL. Paris, Alcan. 1900. Pp. 417.

Wirklichkeiten; Beiträge zum Weltverständniss. K. LASSWITZ. Berlin, E. Felber. 1900. Pp. vi + 444.

Monistische Gottes und Weltanschauungen. J. SACK. Leipzig, Engelmann. 1900. Pp. viii + 278.

NOTES.

MR. WILLIAM RICHARD SORLEY, professor of moral philosophy in the University of Aberdeen, has been elected to the Knightbridge professorship of moral philosophy at Cambridge University, in the place of Professor Henry Sidgwick who has been compelled to resign owing to ill health.

Professor Th. Ziehen, associate professor of psychiatry in the University at Jena, has been called to the University of Utrecht.

W. D. SCOTT, Ph.D. (Leipzig), has been appointed to the newly established instructorship in psychology and pedagogy in the Northwestern University.

DR. GEORGE V. N. DEARBORN has been appointed assistant professor of physiology in the Tufts College Medical School.

DR. S. E. MEZES of the University of Texas has been promoted from an associate to a full professorship of philosophy.

PROFESSOR LUCIANI, rector of the University of Rome, has celebrated the completion of his 25th year of teaching. Exercises were held in the physiological laboratory of the University, and addresses were made by Professors Todaro and Baccelli to which Professor Luciani replied.

ANOTHER eminent Italian neurologist, Professor Camillo Golgi, has been made a senator of the Kingdom.

PROFESSOR J. MARK BALDWIN, of Princeton University, has returned to the United States after a residence of over a year at Oxford, where he had been seeing through the press the 'Dictionary of Psychology and Philosophy' shortly to be published by The Macmillan Company.

PLANS have been proposed for the establishment in Paris of the 'International Society of the Psychical Institute,' the aims of which, according to a circular signed by M. Pierre Janet, are:

1. To collect in a library and museum all books, works, publications, apparatus, etc., relating to psychical science.

2. To place at the disposal of researchers, either as gifts or as loans, according to circumstances, such books and instruments necessary for their studies as the Institute may be able to acquire.

3. To supply assistance to any laboratory or to any investigators, working singly or unitedly, who can show that they require that assistance for a publication or for a research of recognized interest. This function, which has been fulfilled so usefully by the Société pour l'Avancement des Sciences, in relation to the physical sciences, must also be discharged by the new Institute in relation to mental science.

4. To encourage study and research with regard to such phenomena as may be considered of sufficient importance.

5. To organize lectures and courses of instruction upon the different branches of psychical science.

6. To organize, as far as means will allow, permanent laboratories and a clinic, where such researches as may be considered desirable will be pursued by certain of the members.

7. To publish the 'Annales de l'Institut Psychique International de Paris,' which will comprise a summary of the work in which members of the Institute have taken part, and which may be of a character to contribute to the progress of the science.

Founders, donors, and ordinary members are wanted for the Institute, the first class are to give 10,000 fr. or more, the second 1,000 fr. or more, while ordinary members pay an annual subscription of 20 fr. An international council of organization has been formed in which America is represented by Professor James. Professor J. Mark Baldwin, Princeton, N. J., is a member of the committee of patrons, and will send circulars to those making application for them.

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THE PSYCHOLOGICAL REVIEW.

THE FOURTH INTERNATIONAL CONGRESS OF PSYCHOLOGY.

BY PROFESSOR HOWARD C. WARREN.

Princeton University.

The international gatherings of psychologists, which seem now to have become a permanent institution, originated, it will be remembered, with the Paris Exposition of 1889, when the French authorities arranged for a number of congresses representing various sciences, professions and practical pursuits. The same policy was adopted in connection with the Exposition of 1900, and psychologists were again invited to hold an international congress at Paris. Meanwhile, the idea had taken root, and two other psychological congresses had been held elsewhere—in London (1892) and Munich (1896)—making the present session the fourth of the series. It should not be forgotten, however, that the idea originated in France, and that we are under great obligations to the authorities and psychologists of that country for the practical realization of what was long regarded as merely an impracticable ideal. This obligation is now greatly increased, for the hospitality of the French nation and individuals, and the activity of the committee charged with organizing the Fourth Congress, were chief factors in the success of the meeting.

In attempting an estimate of the value of the Congress and its work, much depends on our conception of the functions of such a gathering. Apart from the reading of papers, the discussion and formal interchange of ideas, and the opportunity

afforded of coming face to face with the personalities behind the writings one has admired or criticised, constitute factors of no small importance in the attraction and helpfulness of these meetings. Indeed, in the minds of some, the order of these elements should be the reverse of that just given. The papers presented (on experimental topics, for example) may or may not furnish contributions of importance to the science. As a rule, their contents, if valuable, are not new; or, if new, are not fully appreciated. The most profitable contributions, with some exceptions, are résumés of results which the authors have recently published, and possibly exploited at their local academies or national associations as well. A new monograph is usually better understood when seen in print than when listened to. But there is not the same facility in discussing the printed as the verbal presentation. A magazine discussion has obvious limitations: it takes time to work out, and where several participate it becomes too complex to follow readily. In a meeting, the more salient points may be easily picked out and discussed by a number of speakers, with profit to themselves and to their audience. For this reason the discussion may be regarded as more important than the papers. The social atmosphere is another element to be considered, and not the least important. This is naturally more varied and more stimulating (if the conditions favor) in the Congress than at local or national meetings. The influence of mere personality should not be underestimated—least of all by psychologists.

Two circumstances militated somewhat against the Paris Congress in these respects. One was the absence of a number of recognized leaders of psychological thought, whose attendance had been counted on. The other was the diversity of language, which proved a greater obstacle than might have been expected.

In view of the relative accessibility of Paris and the added attraction of the Exposition, it was generally believed that the Congress of 1900 would surpass even its immediate predecessor, both in numbers and in the quality of its membership and papers. Moreover the tireless efforts of the Secretary, M. Janet, and the Committee who assisted him, left nothing undone to secure this

result. Any lack that may have been felt in the meetings was certainly not due to any omission on their part. But a number of independent causes seemed to have conspired to deplete the attendance. The hot summer, the crowded state of the city, the high price of living, together with social, personal and other reasons, were responsible for the absence of many prominent foreign psychologists, whose names were actually on the rolls. In this respect the Congress was somewhat disappointing, and probably most so of all to the French psychologists, who were present almost without exception.

The barriers of language, too, were surprisingly strong, considering the growing internationality of psychological literature. A majority of the papers were in French, and even that language was unknown to some of those present, while a large number were obviously unfamiliar with some of the other languages used. This hampered the discussion considerably. It would certainly seem that, with the proceedings limited to the four languages in which most psychological material appears, such a difficulty should not have arisen. However, it must be added, in fairness, that the acoustic properties of the large halls were very poor, and many of the addresses were inaudible except to those quite near the speaker. This made it difficult at best to follow the proceedings carried on in a foreign tongue. But the problem of language remains, and it must be met in connection with future congresses. If the discussion is to attain its end, and if it is to prove really beneficial, it is necessary that every speaker be understood by the greater part of the audience. No international congress can be wholly satisfactory till this condition of affairs is reached. Whether a single medium of communication is better than several, is to-day only a forensic question. The modern tendency is to enlarge the number rather than restrict it. The Russians made an earnest plea at the Congress for the recognition of their tongue. And experience there as elsewhere proved that a speaker was generally more fluent and more easy to follow when using his own language than when attempting to speak in another—that is, of course, to one understanding both. But any multiplication of tongues involves a hardship. At present, 'current' literature on psychology is practically confined

to the four languages used at the Paris Congress; the others, notably Russian, are sealed books to most of us. The most obvious step toward promoting the success of future congresses would be for psychologists to improve their knowledge of these four. And American psychologists have at least as much before them in this respect as any others. The question of Russian will doubtless present itself with growing force in the near future.

This apparently minor matter is dwelt on here because of the notable part it played in the Paris Congress. It was responsible for considerable lack of life in the discussion, besides creating some confusion at times. Few of the papers were discussed, except in the speaker's language, by those accustomed to using it. Lines were unconsciously drawn in this way, and the real benefit of an international gathering was thus to a certain extent lost.

A further difficulty was the absence of printed analyses in the case of many papers; the authors were themselves to blame for this, but the effect was felt largely by the audience. The sectional programs, too, were more in the nature of a Chinese puzzle than a circular intended to impart information. The list of speakers was printed in one place, the titles of communications in another, and the whole so arranged that it was impossible to determine the hour or even the day when a given paper was to be read. But as this was the only shortcoming of the committee it may well be passed over lightly.

Despite these drawbacks the Congress was a notable event and will take a worthy place among its fellows. If it failed to realize every expectation, it certainly did amply repay the attendance. The papers were interesting and the discussions profitable. The topics were drawn from a wide field and covered the latest work and thought in all departments. An admirable proportion was maintained between the different branches, proving the gathering to be quite representative in character. There were over 300 members in attendance, drawn from almost every country. France preponderated in numbers, as was to be expected, while the attendance from Italy and Russia was surprisingly large. The German-speaking coun-

tries were very well represented, and the Anglo-Saxon contingent (largely American) was considerable, though not so numerous as anticipated. The other countries showed well in proportion to their size and distance, even South America, Japan and India sending representatives.

The meetings lasted from August 20th to 25th, with two sessions daily; one was a general session, with select papers from all departments, the other consisted of the sectional meetings. All the sessions were held in the Palais des Congrès, a building within the Exposition grounds, designed especially for the series of congresses held during the year. The card of membership gave admission to the Exposition at all times during the week of the Congress, and afforded reduced rates at many of the 'side-shows.' In this and many other ways attentions of the most generous sort were offered, both by the authorities and by the home members, to their guests.

A reception was given to the members of the Congress by Professor and Mme. Richet, another by Prince Roland Bonaparte, and a third by the Institut Psychique at its headquarters. On two evenings the Committee provided complimentary tickets to theatrical performances in the Exposition. On Thursday a visit was made to Dr. Sollier's Sanatorium, at Boulogne-sur-Seine; and on the 26th Dr. Toulouse invited the Congress to visit the Asylum at Villejuif, and, after showing the guests through the grounds and buildings, gave a successful demonstration of various types of insanity, by practical examples chosen among his patients.

The subscription banquet, Saturday evening, followed the last of the meetings, and was a very pleasant affair. It was held on the first landing of the Eiffel Tower, where the delightful outlook and cool air assisted in making it a fitting termination of the week's labors. The usual complimentary speeches were made by Messrs. Ribot, Ebbinghaus, Sergi, Janet, Lemon, Prince Tarkhanoff and others.

As already said, all sides of psychological thought and investigation were well represented at the Congress. No single subject formed the 'key-note' of the meeting. Undoubtedly the greatest emphasis was laid on problems of theory and nor-

mal experimental research; but psychical research was also quite prominent. The theoretic problems of psychology received perhaps most attention, all in all. The Section on Introspective Psychology and its Relation to Philosophy (Section II.), presided over by Professor Séailles (Sorbonne), furnished the largest number of papers, and as a rule these were ably written and worthy of careful consideration. The attendance in this section was greater, on the average, than at any of the others. The historical papers of Professors Ribot and Ebbinghaus were presented in general session.

Experimental research may reasonably claim the next place. In addition to the numerous contributions at the special Section on Experimental Psychology and Psycho-physics (III.), under Professor Binet (Sorbonne), one general session was devoted exclusively to papers of this character. The contributions were numerous and covered a great variety of topics, among which may be noted: psycho-physical method, skin sensations, vision, muscle sense, space perception, association, memory, dreams, fatigue, pain, æsthetics and movement. They reflected a healthy activity in research work all along the line, although no discovery of the first magnitude was brought to light. The reports generally embodied the results of careful investigation; taken together, they represent, so far as the judgment of more critical examination can be anticipated, a distinct contribution to the archives of experimental psychology. The lack of linguistic unity was especially felt in this section; it interfered with the flow of discussion at points where criticism and reply would have been most profitable to all concerned. So far as it went, the discussion was lively and stimulating.

The interest exhibited in psychical research and spiritualism deserves mention, whatever attitude be taken towards the subject. At the general session set apart for these topics the attendance was apparently at high-water mark. The papers and remarks of Messrs. Flournoy, Myers and Van Eeden commanded general attention, while the presence of the new 'medium,' Mrs. Thompson, did not fail to attract. The most dramatic feature of the Congress also occurred at this session, when Jagadiska Chandra Chatterji (Benares) made an elo-

quent plea for the study, by Western investigators, of the Hindu psychological methods, which deal largely with telepathic phenomena. It would be unfair, however, to lay too much stress on the momentary popularity of this branch at the Congress. The papers, though excellent, were few in number; while many of those attracted to this meeting were 'lay members,' whose interest in matters psychological appeared to be confined to this particular topic. The Section devoted to Hypnotism, Suggestion and Cognate Subjects (V.), under Dr. Bernheim (Nancy), included only a few papers on psychical research.

The other sections were variously attended; as in the departments already mentioned, the papers showed that a satisfactory course of progress was being maintained in these lines. Sections I., on Psychology in Relation to Anatomy and Physiology, and VII., on Comparative Psychology, met in joint session, under Professor Delage (Sorbonne). Section IV., on Pathological Psychology and Psychiatry, was presided over by Dr. Magnan (Paris Academy of Medicine). Section VI., on Social and Criminal Psychology, under Professor Tarde (Collège de France), was least representative, on account of the special congresses on each of these sciences held in Paris during the summer. The work of these sections was supplemented at two of the general sessions.

To speak here of all the 150 or more papers presented at the six general and twenty sectional meetings is manifestly impossible, and it would be difficult to deal fairly with all the sections in a rapid survey. But some of the papers on theoretic and experimental topics should at least be mentioned.

The opening address, by the President, Professor Ribot (Collège de France), was a well-drawn sketch of the development of psychology in the eleven years since the first Paris Congress. Following this was a paper by Professor Ebbinghaus (Breslau), in which he compared psychology at the present day with the state of the science 100 years ago. Both of these addresses were optimistic in character, and formed a suitable introduction to the work of the Congress.

Among the papers dealing with fundamental problems, the most speculative was that of Dr. Aars (Christiania), entitled

'Sieben Rätsel der Psyche.' He dwelt especially on the inexplicable nature of the relation between past and present in consciousness, the irreducible character of sensation, feeling and conation, the antithesis between the processes of assimilation and comparison, and the problem of the projection of subjective experiences into the outer space-world.

In a paper on the relation between mind and body, Dr. Frésic Pavicié (Zagreb) proposes to regard these two, not as distinct in character, with the Dualists, nor as identical, with the Monists, but as constituting the two extremes of a graduated series of forces, the intermediate terms of which are as yet unknown to us. He instances the animal and vegetable worlds as such a series; among their rudimentary forms, the differences are indistinguishable; in their higher developments they are, nevertheless, so distinct that any connection would be impossible to imagine, had we not the intermediate forms before our eyes.

An interesting application of psychological notions to the natural history of philosophy was made in a paper by Freih. v. Ehrenfels (Prag): Die biologische Wurzel des Positivismus. He argues that, from a biological standpoint, the positivistic tendency is better adapted to the proper functioning of the *Erkenntnistrieb*, than the metaphysical; it enables us to systematize our knowledge and purposive activity more easily, since it bases the working of the cognitive instinct, in every case, directly on the category of the particular cognition concerned, instead of regarding the latter as derivative. It has therefore a favorable prospect for ultimate survival. Nevertheless, the tendency to speculation is an instinct firmly embedded in human nature, and apparently too deeply ingrained to be uprooted completely. Even an unfavorable biological instinct may bring to light true knowledge.

Professor Marty (Prag), discussing Resemblance, pointed out two distinct meanings of the term: 1. Things which differ so slightly that they are liable to confusion, are called similar; 2. Things which recall one another on account of their agreement. Negatively, the two meanings may be combined: Resemblance is said to exist between things only where the dif-

ference is not so great that the one can no longer recall the other. One thing may recall another without any liability of confusing them. A lesser difference, however, constitutes a *higher degree* of resemblance, and the highest degree of all is where the difference is so slight that one is extremely liable to be mistaken for the other. Resemblance, in either sense, may occur either through partial identity, or without any such identity, as between two species of the same genus. Finally, resemblance is always a matter of immediate perception.

Mlle. Marie Bœuf (Paris), in a contribution to the psychological theory of time, distinguished between *psychological* time (a quality, the degree-of-presence tone), and *linear* time (a quantity, the length and relative positions of events). "Psychological time rests on an intuition;" that is, "the sensation of time originates in the organism"—it is furnished by the organic sense (sens interne); in its most primitive form, it is the consciousness of nervous rhythm. From this standpoint, the unit of time is the unit of concentration; when the nervous rhythm is accelerated, the unit of time becomes, not longer, but more 'saturated,' and *vice versa*. This unit varies from species to species; there is, apparently, a tendency to acceleration, in the course of history. The immediate datum is qualitative. The mental *projection* of events along an *extended* time is a subordinate process. Such localization is always with reference to the psychological present; it is merely a translation of nervous tensions "into feelings which give us the various degrees of presence." "The localization in *linear* time, on the other hand, proceeds by means of reference points outside of the self." The conflict between these two schemes gives rise to illusions of time.

Speaking on the Definition of Perception, Professor Claparède (Geneva) began by calling attention to the different usage between English and French on one side and Germans on the other, in the method of subdividing mental phenomena—the former making the distinction between presentation and representation the test, the latter that between simplicity and complexity. Leaving aside questions concerning the genesis of our belief in the external world, the chief characteristics of per-

ception are: 1. That the sense impressions and associated images involved in perception constitute a mental entity. 2. That this mental entity is perceived as not belonging to the self. But mental phenomena include not only the *perception of spatial objects*, but also *mental operations* performed on sense impressions (comparison, sense of duration, etc.). Should the term *perception* be limited to the former class of facts—in which case we could no longer speak of perceiving a difference, or perceiving time? Or should its scope be enlarged so as not to involve any necessary implication of objectivity? If no satisfactory agreement can be reached on this point, it would at least be useful to know how each particular author employs the term.

In a paper entitled: Le problème de la conscience dans la psychologie expérimentale, Dr. Philippe (Sorbonne) calls attention to the growing discord between the reports of the individual consciousness and the result of experimental investigation. The length of reaction-times is grossly misjudged; we are 'conscious' of loss of consciousness when only a loss of memory has occurred, etc. Is the science reaching a point where "consciousness must be denied even the cognition of those modifications which constitute its sole *raison d'être*"? The speaker believes that the difficulty arises rather from the faulty classification of mental phenomena which has been handed down to us, and which attributes, not too much, but rather *wrong characteristics* to consciousness.

Among other papers presented may be mentioned those of Professor H. Bergson (Collège de France), on the consciousness of intellectual effort; Dr. Kreibig (Vienna), on the notion of the 'Sinnestäuschung'; Professor von Tschisch (Jurjeff), on theories of pain; Professor Tisserand (Bourges), comparing the Herbartian with the so-called physiological theory of pleasure; and Dr. Hartenberg (Paris), on the psychology of timidity. Professor Münsterberg (Harvard), who was not down on the program for a paper, also made an address.

To pass on to experimental papers. N. Vaschide (Rumania) reported the results of numerous experiments bearing on the relation between muscular and tactile sensibility. The phe-

nomena investigated were: (1) anatomical and physiological; (2) pathological, and (3) psychological. The general conclusion, which all the results seemed to substantiate, was that the muscle sense is distinct and irreducible; its organ is the muscle itself; and it acts, sensorially and intellectually, like all other senses.

S. Alrutz (Upsala) reported several curious phenomena relating to the temperature senses, distinguishing between warmth, cold, heat, pain, burning and smarting sensations.

Dr. P. Mentz (Leipzig) indicated the results of experiments on the comparative saturation of various parts of the spectrum, by direct comparison, using the Method of the Fourth Proportional, with differences slightly above the threshold. The points of maximal saturation thus obtained coincide with the 'purest colors' obtained with other well-known methods. The number of maximal points is of considerable theoretic interest; here, however, the disturbing effects of suggestion and contrast were felt, as well as an uncertainty arising from the difficulty of determining the *direction* of difference with such slight differences as those used. Apart from this, the position of the maxima along the spectrum was satisfactorily determined. The method, said the speaker, might with advantage be used with the doubtful colors in cases of color-blindness. In the same line, Professor Scripture (Yale) exhibited and demonstrated his new apparatus for testing color-blindness.

A paper by Professor Stratton (California), read in his absence by Dr. Woodworth, reported the determination of the *minimum visibile* by a new method. Lines were placed end to end instead of side by side. The threshold value was found to be about 7″ of arc instead of 50″–60″, as given by the usual method. These results seem to make it "necessary to modify our conception of the spatial signs here, since the above measurements are considerably less than the distance apart of the sensory elements in the retina, and less than can well be accounted for by the muscular apparatus of the eye."

L. Marillier and Dr. Philippe (Sorbonne) announced a set of new æsthesiometric measurements. These were performed on four subjects, over the entire body, so as to furnish a complete to-

pographical chart of space sensibility. Weber's compasses were used, with points of various forms. The measurements were taken along two vertical lines, one on each side of the body, in front, reaching from the shoulder to the great toe; two corresponding lines on the back; an internal and external line on each arm, and two median vertical lines, back and front. The hands and feet were carefully explored, and in other regions transverse measurements were taken in sufficient number to indicate the entire topography. Incidentally, it was found that with two compass points of different character the subject observed a distinction at distances far below the ordinary threshold; the points were often perceived as qualitatively distinct, even where they were not locally distinguished.

In connection with these researches may be mentioned the paper of Professor Külpe (Würzburg) on the relation of the least perceptible difference to differences above the threshold.

Professor Monroe (Westfield, Mass.) reported experiments proving the existence of olfactory images in dreams. Professor Tarkhanoff (St. Petersburg) described certain hallucinations observed in frogs under the influence of ether.

Mlle. Joteyko (Brussels) presented two papers on the subject of fatigue. Her experiments with the ergograph led to the conclusion that 'the motor centers resist fatigue incomparably better than the terminal organs.' The theory of the central origin of motor exhaustion is therefore not confirmed experimentally; all the researches pointed to the peripheral origin of motor fatigue —through the exhaustion, not of the contractile substance, but of the intra-muscular nerve-endings. In her other paper, Mlle. Joteyko applied these results to a theory of the biological value of fatigue, as a means of defense on the part of the organism. Three modes of defense were noted: immediate (peripheral paralysis), preventive (sense of fatigue), and consequential (habituation, rendering the organism more capable of resisting fatigue).

Professor Sommer (Giessen) demonstrated several new apparatus. He exhibited two pieces for the graphic representation of movements in all three dimensions—one for the finer movements of the finger, the other for movements of the lower limbs.

A third apparatus was designed to measure the pupillary reflex under varying degrees of light stimulation.

In the line of Child Psychology, Professor Netchaeff (St. Petersburg) reported a series of observations on the development of memory among school children. Six hundred and eighty-seven scholars of both sexes, between the ages of nine and eighteen, were examined. The tests included memory for objects, sounds, numbers and words representing various classes of sensations and abstract ideas. A steady growth of memory with years was observed in almost all these respects, which was somewhat checked, however, during the period of puberty. A marked difference was found between the various classes of stimuli, and according to the meaning of the word. The boys showed a relatively better memory for objects and sounds, the girls for numbers and words.

N. Vaschide reported some experiments and observations on the constructive imagination in children. The principal subject was carefully studied from the age of six months to four years. The results obtained from him were supplemented by observations on twelve children of both sexes from three to four years of age.

Professor Bryan (Indiana) reported his experiments upon the recently discovered arithmetical prodigy, and Professor Richet (Paris) exhibited a remarkable musical precocity of the age of four.

Miss Paget and Miss Anstruther-Thomson (London) reported the results of an investigation on the rôle of the motor element in the æsthetics of vision. They found that the æsthetic pleasantness or unpleasantness of visual objects depended not only on the activity of the visual organ and the associated muscular processes, but also on the functions of respiration, circulation, equilibrium and internal motor adjustment. The sensations accompanying these are not the effect of the emotion, but rather its cause and explanation, or possibly they constitute the emotion itself. They accompany the activity of the eye, giving the qualities of linear direction and space relation, and all the rhythmic qualities of visual space generally. The presence of these motor elements is discovered by the examination

of various sorts of sensation, some of which are apparent in the metaphors applied to visual sensations, while many others can only be brought to light by experiment. The speakers presented a *questionnaire* on the topic, by means of which they propose to enlarge the scope of their investigation.

The next Congress was announced to meet at Rome in 1904, under the presidency of Professor Sergi, and an international committee was formed to organize and carry out the arrangements.

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MENTAL FATIGUE. I.

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II. Mental Fatigue in School Children.

It has been generally supposed and by some investigators asserted as proved that the child in school is rendered by the work that he does in a single session less able to do mental work. Having worked say an hour, he can, we are told, do during the next hour less work. Attempts have been made to measure this decrease in ability to do mental work due to the work of an hour or a session. Various methods have been used and various results obtained. Some of these have served as excuses for recommendations of shorter periods, longer pauses, etc., etc. There has been, however, about methods, results and recommendations no decided agreement, and the whole matter of mental fatigue in schools is still a problem.

The present research aims to settle the leading question in this problem, the question, namely, of how much less able to work the child is after having done the work of half or the whole of a school day than he was at the start. The results, which will be presented in detail, are unanimously in favor of the answer, "*He is just exactly as able.*" If these results were legitimately obtained, they prove that the work in the case of the schools tested *did not decrease one jot or tittle the ability of the scholars to do mental work.* The rest of this report will, besides giving these results, explain minutely the methods employed, the conditions of their administration, the methods of computing the results and the bearing of our answer to this fundamental question on the whole school fatigue problem.

There are four principal dangers to be guarded against in attempts to investigate and measure changes in the ability to do mental work. First and most important there is danger lest

you measure not the ability but the willingness to do work. The mere fact that a person does less work may as well mean that he did not want to do more as that he was not able to do more. Secondly, there is danger lest you measure not the decrease due to inability, but the decrease due to inability *minus* an increase due to practice. If the same sort of test, *e. g.*, multiplication, is given in the morning and again at noon, the morning practice may improve the quality of the noon test. Thirdly, there is danger lest emotional excitement interfere. Thus the novelty of a first test might improve or injure the quality of the work done in comparison with that of a second test. Fourthly, there is danger that in very subtle ways by differences in the manner of giving instructions to the pupils the nature of the work may be altered.

To escape from all these dangers the following method was devised: The tests of ability to work were not given by the regular school teachers or as a professed school task, but were given by two persons only, both of them strangers to the school children. Thus all influences of conventional habits of working better at one time than another, of repugnance to school tasks, of ennui, of dislike for the teacher, etc., were eliminated except in so far as such lay in the tests themselves and the mental condition of the pupils. Thus we avoid the first danger and measure not the willingness to work for a teacher at certain times in the day, but something at least very near the actual ability to work.

The influence of practice was avoided by never giving the same sort of test to any child twice. Instead of giving the same lot of children a test in multiplication early and another test late, I gave the same multiplication test to one lot of children early and to another lot late in the session. This of course utterly removes practice from the question, but introduces another disturbing factor, differences in the average ability of the two sets of children. This factor did not, however, disturb anything, for: (1) children were taken from the same school grades, and (2) enough were taken to insure approximately equal ability in the two sets, and (3) several different tests were used, so that the set of children which had one kind of test early in the day had

another kind late in the day, and vice versa. Thus the difference in ability, what little there was, counted as much for the early as for the late side, and its presence could be readily detected. This feature in our method is, so far as I know, new and seems worth adopting, not only because it eliminates the influence of practice, but also because it eliminates the contrary influence of possible ennui at doing the same sort of work a second time.

In order that the third influence mentioned, that of emotional excitement at the first visit of a stranger, might not disturb the results, half of the first visits were made early in the school day and half late, so that this influence, if there be such, would affect early and late tests equally. Finally the instructions were, except in the case of about one-twentieth of the children in one single test, given after a regular model, so that chance changes in them might not work to favor either the early or late tests.

As has been stated, a number of different tests of ability to do mental work were used with each class, so that no peculiarity in the nature of the work might render the results unrepresentative of ability to do mental work in general. As will be later seen the large number of children tested insures that the results are not due to individual eccentricities. I may also add here (some things in previous studies of fatigue make the statement necessary) that no child was excused from the tests except for inability to write, that no papers handed in were discarded in reckoning up results save such as are explicitly mentioned, and that all misapprehensions of the tests are noted in the results or otherwise referred to in the text.[1] The results are from school children in general, not from particular school children picked out for the purpose.

The first kind of work given as a test was multiplication. The scholars were given a certain time in which they were told to do as many as they could of the following examples, which were printed on sheets of paper and passed around. Sometimes sheets containing only three or six examples were used, but there was an equality between tests early and tests late in

[1] Save that in *one early* test, 3 papers in multiplication where the children had *copied off* the examples before doing them were not counted.

the day in this respect. The time given was in general short enough to prevent all but one or two of the very quickest workers from finishing the work. (A full account of the administration of these tests will come later.) The following is a facsimile of the set generally used.

MULTIPLICATION EXAMPLES.

7986	7869	9867
4523	5324	3425
8679	7968	7698
3542	3254	5423
8967	7896	6493
4532	5243	8786

The second piece of work was to mark as many of the misspelled words in the following passage as was possible within a certain time. As in test 1, the time allowed was so short that only the very best pupils finished. No perfect paper was handed in.

MARK EVERY WORD THAT IS NOT SPELLED CORRECTLY.

1. On the 3d of September, 1832, inteligence was broght to the collecter of Tinnevelly that som wildd eliphants had appeared in the neighborhod. A hunting party was imediately formed, and a large number of nattive hunters were engaged. We left the tents, on horsback, at half-past sevin o'clock in the morrning and rode thre miles to an open spote, flanked on one sid bye Rice-fields, and on the other by a jungle.

2. After waiting som time, Captain B——— and myself walked acros the rice-fields to the shad of a tree. There we herd the trumpett of an elephant; we reshed acros the rice-fields up to our knes in mud, but all in vaiu, thogh we came upon the trak of one of the animels, and then ran five or six hundredd yards iutoo the jungle.

3. After varius false allarms aud vane endevors to discuvor the obgects of our chace, the colector went into the jungle, and Captain B——— and myself into a bed of the stream' where we had sen the traks; and here it was evedent the elaphents had passed to and fro. Disapointed and impasient, we allmost determened to giv up the chace and go home; but shots fird just before us reanimated us, aud we proceded, and found the collecter had just firred twicce.

4. Of we went throuh forest, over ravin, and through strems, till att last, at the top of the ravine, the eliphants were seen. This was a momant of excitment! We wer all scatered. The collecter had taken the midle path; Captain B———, some huntsmen, and myself took to the left; and the other hunters scrambled down that to the rite. At this momunt I did not see enything but after advanceing a few yards, the hugh hed ef an elephunt shaking abuve the jungle, withen ten yards of us, burst sudenly upon my view.

5. Captain B——— ande a hunter justt befor me; we al fired at the same moment, and in so dirrect a line that the percussion-cap of my gun hitt the hunter, whome I thougt at first I had shoot. This acident, thogh it prouved slight, troubled me a litle. The

grate excitment ocasioned by seeing, for the first tim, a wild best at liberty and in a state of natur, product a sensation of hop and fear that was intens.

In the third test the scholars were required (*a*) to write from memory (after seeing them on a chart for ten seconds) the following figures:

7 5 8 4 2 9 6 1 3 4

and (*b*) to do the same with the similar set

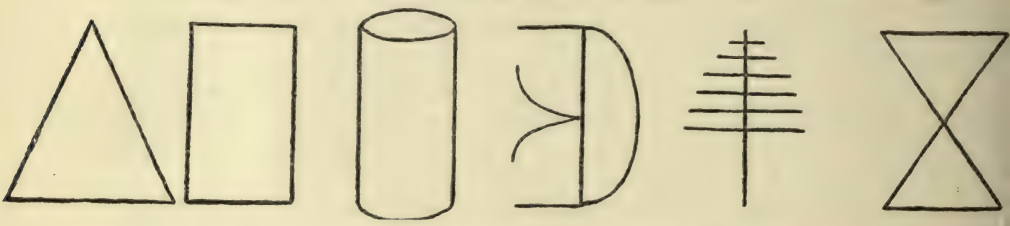
8 2 7 3 6 4 9 5 1 9

In the fourth test the same thing was done with the following sets of nonsense syllables. These were written in small letters on two charts.

(*a*) ba ni su et ko
(*b*) ig fa tu le ro

In the fifth set the scholars drew from memory the following figures (after ten seconds' exposure on a small chart).

FIG. 1.



The sixth test was like the third, except that the numbers were

(*a*) 4 2 6 9 7 3 8 5 4 6
(*b*) 8 4 2 5 9 7 3 8 9 11

The seventh test was like the third and sixth, except that letters were used instead of figures as follows:

(*a*) P V R E X O A S T I
(*b*) R S K X A N C M L

In test eight the scholars counted the number of dots on a chart which was exposed five seconds.

These tests would seem to be fair measures of the ability of school children from the fifth to the seventh grades to do mental work. The speed and accuracy of the work of the first two will depend, other things being equal, upon the readiness of associations, their accuracy and the amount of attention or concentration bestowed upon the work. The accuracy of the work in the other tests will vary with the intensity of the attention given to the charts, and the success of the pupils in holding the figures, etc. in mind long enough to write them down. The results would not show mental inability so decidedly or delicately as the mental multiplication of fairly large numbers, but for class tests they should answer. In tests one and two less time was given than was necessary to do the examples, so that the amount done as well as the accuracy of the work affords a reliable measure.

Finally, although every precaution possible was taken *not* to have the work done as a regular school lesson, these tests are of the same *kind* of ability as school work requires. If a class does this sort of work as well early as late, it can do its other lessons as well so far as mere ability goes. The mental work was not peculiar or the powers required foreign to ordinary school life.

Before explaining how the quantity and quality of the work were computed it may be well to give a summary of the results obtained with each test.

Test 1. Those who did the multiplication work late in the day did 99.3 per cent. as much and made 3.9 per cent. more mistakes than those who did it early and numbered 64 who misunderstood or grossly failed in the test against 56 among the early ones.

Test 2. The spelling test, which was given early to those who had test 1 late, and vice versa, shows the following results for those who took it late as compared with those who took it early.

Relative percentage of page covered, 99.0 per cent.

Relative number of words marked, 105.0 per cent.

Relative number of words marked improperly, 97.9 per cent.

Thus the decrease in ability shown in test 1 is offset by an equal increase in the case of test 2.

The third test was given to four classes early and four late; the sixth test likewise. Taking both together we find that the pupils who did the work late in the forenoon or late in the afternoon did almost 2 per cent. *better* than those who had the work early. But with tests 4 and 7, where the pupils were reversed, we find the late pupils doing only 98 per cent. as well as the early with test 4 and 99.8 per cent. as well with test 7. So here again the balance is practically equal.

Test 5 was given late to only half the total lot of pupils and the set who took it early were shown by the other tests to be a bit the more intelligent. Here the late pupils did only 94.6 per cent. as well. That this was wholly due to a difference in average ability is also witnessed by the fact that when 75 per cent. of these scholars were given test 8 (those who had 5 early taking 8 late, and vice versa) the scholars taking the test late did much more than five per cent. better. No exact comparison can be made because only six classes out of eight who had test 5 were given test 8.

We have now, after describing the nature of the method in general, given the grounds for our first statement that the child in school could work exactly as well late in the forenoon or afternoon as early in the forenoon. It now becomes our duty to show that these percentages are the legitimate outcome of the particular data and that the tests were so administered that the particular data are reliable.

The particular data are of course the papers handed in by the children. In test 1 we have papers with more or less multiplication work on them. We have to keep account of the amount of work done, the number of mistakes made and of any special blunders. In reckoning the amount of work I took as a unit one of the four products which, when added together, give the answer. Thus a pupil doing one example was scored 4, one doing two examples was scored 8, one doing two examples and two lines of another was scored 10. This is obviously not accurate as it disregards the work of addition, but with over 375

students in both early and late tests it is impartial and is more accurate than a scoring by finished examples. And if at any time the method is questioned one can readily turn the results into double records of the number of examples finished and the number of lines done besides. I am sure no change would be found in the relations of the results of the early test to those of the late. In reckoning the number of mistakes I reckoned in the case of the finished examples only mistakes in the answer, and in the case of unfinished examples all mistakes in the work. Each wrong or omitted figure was counted one mistake, save that where two wrong figures might have been due to a mistake of one or two in the work (*e. g.*, 80 for 79, 61 for 59) I called them one mistake. Special blunders occurred to a considerable extent in the lowest grades tested, where the pupils had evidently not thoroughly mastered multiplication. Some added, some subtracted, some multiplied the multiplicand by only a single figure of the multiplier, some started to add and then multiplied, some started to subtract and then multiplied. Record was kept of all such, and the 58 and 64 in the summary of results refer to these. One sort of mistake might be recorded by itself, but was made to equal five regular mistakes by following the regular method. I refer to the omission of the last partial product, an omission found in very good papers both early and late. As it was not a miscomprehension but just a grave blunder I in every case reckoned the example as finished with five mistakes. The records of the individual pupils were added together by classes. We have thus the following table (I.).

Now before comparing the totals it is necessary to reduce the early and late work to an equivalence in the number of students. In doing this it is not enough to add all the early and all the late pupils and to subtract from the work done and mistakes made by the larger number amounts proportional to the excess in the number of scholars, for 5th, 6th and 7th grade students are not on an equality in this regard. Consequently we have to treat the work of the students of the different grades separately. The most satisfactory way seemed to be to estimate the work of 37 of the 40 students in J so as to have J compare with V; to estimate the work of 29 of the 32 students of O, of

TABLE I.—MULTIPLICATION.

EARLY; *i. e.*, done between 10 and 40 minutes after school opened in the morning.

Time given for Test.	Class.	Grade.	Number of Scholars.	Amount of work done.	Number of Mistakes.	Number of Scholars who added.	Number of Scholars who subtracted.	Number who started by adding but changed.	Number who started by subtracting but changed.	Number who multiplied by only one fig.
4 min.	O	Cleveland 5th	32	259	91					
5 "	N	" 7th	38	491	132					1
4 "	K	" 6th	42	395	87					
5 "	J	" 7th	40	546	127					
6 "	H	Scranton 5th	44	225	95	5	5			6
" "	I	" "	40	482	111					3
" "	F	" 6th	35	239	78	1	1			4
" "	G	" "	37	460	146					1
" "	A	" 5th	45	214	92	5	5	4	5	
" "	D	" 6th	36	276	76	7		5		
										Total = 58

LATE; *i. e.*, done between 10 and 40 minutes before school closed in the forenoon or afternoon.

Time given for Test.	Class.	Grade.	Number of Scholars.	Amount of work done.	Number of Mistakes.	Number of Scholars who added.	Number of Scholars who subtracted.	Number who started by adding but changed.	Number who started by subtracting but changed.	Number who multiplied by only one fig.
Same as O	T	Cleveland 5th	29	254	79					
" " N	M	" 7th	38	561	98				1	
" " K	L	" 6th	40	386	121					
" " J	V	" 7th	37	622	135					2
" " H	R	Scranton 5th	43	319	138	3		1	1	4
" " "	P	" "	42	210	115	6	2			2
" " "	S	" 6th	32	299	109					5
" " "	Q	" "	37	323	132					
" " "	B	" 5th	42	74	34	1	7	3	6	11
" " "	E	" 6th	37	299	88	5	0	3	1	
										Total = 64

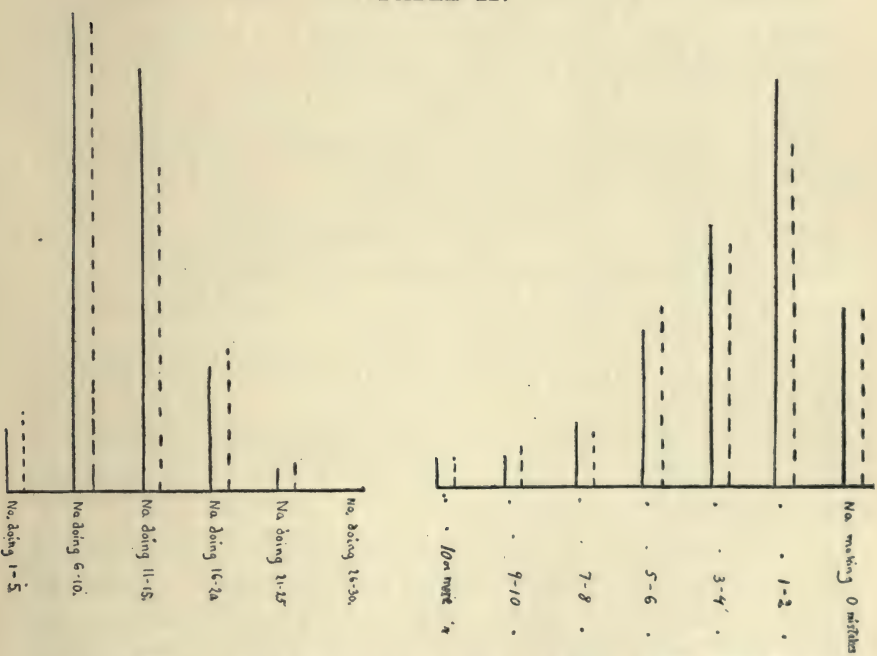
40 of the 42 students of K; to estimate the work of 84 students of the 85 of R and P, of 32 of the 35 students of F, of 42 of the 45 students of A and 36 of the 37 students of E. The reason for not ranking classes A and E with the other Scranton 5th and 6th grades is that they happened to have been already computed. It makes practically no difference. The method of estimating was to deduct in the case of each of the above sets from the amount, mistakes, etc., a percentage equal to the percentage of students deducted. We now have equivalence in the number of students and can add up the work, mistakes, etc., and by doing so we get the following results:

375 early students (estimated from 389) did *3,359* units of work with *1,004* mistakes and *56* very bad papers (*e. g.*, addition instead of multiplication); 375 late students (estimated from 377) did *3,333* units of work with *1,044* mistakes and *64* very bad papers.

From these figures we derive the percentages already given, viz., that those having the test late in the day did 99.3 per cent. as much work, made 103.9 per cent. as many mistakes and numbered besides 64 very bad papers as against 56 in the case of those taking the test early.

Besides this comparison of the total work done I computed the number of those who in the early test did from 1 to 5, 6 to 10, 11 to 15, 16 to 20, 21 to 25, etc., units of work, and similarly of those who took the test late in the day. This was done, of course, in order to detect any phenomena, such as a betterment of the good work offset by a depreciation of the poor work, or

TABLE II.



The continuous lines represent the number of students in the early tests and the broken lines those in the late tests who did 1–5, 6–10, 11–15, etc.

vice versa, which the mere totals would not necessarily show. A similar computation was made of the number of mistakes. The results follow in Table II. It is evident that the percentages of the totals hold also for the students of different degrees of ability and advancement.

In computing the work in the spelling test we have to note not only the number of words marked, but also the number marked that should not have been and the number of lines covered by the pupil. The last thing is particularly important, as it is better work for a student to have marked 50 words in the first 15 lines than in 20. It shows more care and would require more time of anyone, other things being equal.

In recording the number of lines traversed by the pupil, I took the number from the beginning to the lowest line containing a mark, but did not count the sixth line 'the other by a jungle' nor the eleventh line 'the jungle,' nor the last line 'that was intense.' The incomplete lines beginning 'and we proceded' and 'yards of us' were however reckoned as full lines. Here and there I found a paper where the child had either marked the places of the letters omitted and the wrong letters or re-spelled the word correctly. These papers (about a dozen in all, I think) I threw out entirely. I am sure that they were not peculiar to either early or late work. It was a blunder not to keep separate records of all such, and if anyone thinks they would alter the results I will go through the 700 and more papers and look them up, but unless some one judges it necessary it seems a mere waste of time. There was one other misconception, a single case, where the scholar did nothing but fill out the three Captain B———'s with Braddock. This I recorded as no lines done, no words marked. By adding up the number of lines done, words marked (whether rightly or wrongly) and words marked wrongly in each class tested we get the results given in Table III.

From these results there was estimated (in the same way as in the multiplication test) the work of 335 scholars who did the work early and 335 scholars who did it late. The reductions were very slight with the exception of E, where the work of 17 pupils was estimated from that of 40. The absence of a score

TABLE III.—SPELLING.

EARLY; *i. e.*, done between 10 and 40 minutes after school opened in the morning.

Time given for Test.	Class.	Grade.	Number of Scholars.	Number of lines done.	Number of words marked.	Number of words marked wrongly.
1 min. 45 sec.	T	Cleveland 5th	29	216	424	2
5 min.	M	" 7th	42	943	2816	35
1 min. 30 sec.	L	" 6th	42	319	756	6
3 min.	V	" 7th	39	493	1518	25
4 min.	R	Scranton 5th	36	555	1035	37
"	P	" "	37	438	697	6
"	S	" 6th	29	529	978	24
"	Q	" "	33	595	1208	25
"	B	" 5th	46	708	1419	13
"	E	" 6th	40	679	1503	18
Total number of Scholars.....373						

LATE; *i. e.*, done between 10 and 40 minutes before school closed at noon or in the afternoon.

Time given for Test.	Class.	Grade.	Number of Scholars.	Number of lines done.	Number of words marked.	Number of words marked wrongly.
Same as T	O[1]	Cleveland 5th	29	207	507	2
" " M	N	" 7th	37	794	2189	25
" " L	K	" 6th	40	341	848	6
" " V	J	" 7th	40	608	1739	44
" " R	H	Scranton 5th	44	467	834	32
" " "	I	" "	30	463	879	10
" " "	F	" 6th	34	507	1153	33
" " "	G	" "	20	411	898	13
" " "	A	" 5th	45	785	1711	14
" " "	D	" 6th	17	301	720	13
Total number of Scholars.....336						

of pupils who were present during the multiplication test leaves a chance for an accidental error. If, that is, the 17 were not truly representative of the 37 who did the multiplication, we may have a slight variation due to their peculiarities. That is, the 335 late's *may* here not exactly represent the 375 early's of

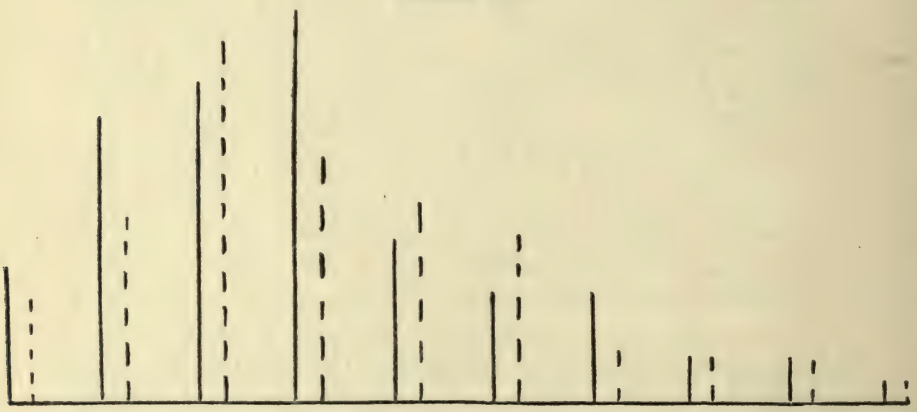
[1] By a mistake test O was given 2 min. 5 sec. instead of 1 min. 45, the results being 247 lines, 604 marked, 2 marked wrongly. I have subtracted $\frac{20}{125}$ from each, which may introduce a very slight error, as the work of the last 20 seconds might be at a different rate from that of the rest.

the multiplication test. Of course we have elsewhere not an exact representation of the students who took one test late in those who took the converse test early, because chance absences, etc., make a variation. But it is not worth while to discuss the possible error due to this, as it is in any case so slight. The totals for the spelling test were found to be

Early:	4,917 lines done,	10,482 words marked,	195 marked wrongly.
Late:	4,871 " "	11,385 " "	191 " "

From these totals we get the percentages already given, viz., that the late scholars covered only 99 per cent. as many lines, but marked 105 per cent. as many words and marked only 97.9 per cent. as many wrongly. The number of words marked was also used as a basis of tables showing the number of early and late students who marked from 1 to 10, 11 to 20, 21 to 30, 31 to 40, 41 to 50, etc. As in the case of the multiplication test, this method of treating the results shows that the mere comparison of totals gives a true view of the case. See Table IV.

TABLE IV.



The continuous lines represent the number of early students, the broken lines, of late students.

In the third and sixth tests the only record made of each paper's work was of the number of figures correctly given and correctly placed. The general rule was to regard as correct any figure which was in the right place *counting from the be-*

ginning, but it seemed more just and was convenient also to score as correct figures that were *continuously* correctly placed counting from the end. Thus (the model being 7584296134) *758427354* was scored 6, *75878134* was scored 6, *7592481734* was scored 4, while 3758429 would be scored 0. The method is not a complete measure of the quality of work, but it is impartial. The series *b* in test 6 was peculiar in having 11 as the tenth number. This was scored as two one's. The exposures in the case of test 3*a* in one early 5th grade class and one late 5th grade class were five instead of ten seconds. The totals for each class were of course obtained by adding the individual scores. They are presented in Table V.

TABLE V.—FIGURE TEST.

	Early.			Late.		
	Class.	Number of Students.	Number of correct figures written (a) and (b) together.	Class.	Number of Students.	Number of correct figures written (a) and (b) together.
Test 3.	T	29	326	O	30	242
	M	42	592	N	36	390
	L	42	453	K	39	410
	V	39	374	J	40	423
	H	44	332	R	43	383
	I	40	311	P	41	419
	F	35	341	S	31	347
	N	37	436	Q	37	464
	Total	308			297	

From the totals for 308 early and 297 late scholars we estimate the totals for 295 early and 295 late scholars of equal school advancement in the same way that we did in the case of the multiplication test, and find that those doing the work early remembered correctly 3,006 figures, while those doing it late remembered 3,060. From these figures we get the relative amount of work of the late pupils, *i. e.*, 102 per cent.

The individual papers written in connection with test 4 (nonsense syllables) were scored in the same way as those of tests 3 and 6. Everything except the correct spelling of a nonsense syllable was marked wrong. To be scored as correct a syllable must be written correctly and either be placed correctly

counting from the beginning, or be one of a *continuous* series of correct syllables counting from the end. The totals by classes are given in Table VI.

The work of test 7 was reckoned up in just the same way as that of tests 3 and 6. The totals by classes are given in Table VI.

TABLE VI.—NONSENSE SYLLABLES AND LETTERS.

Nonsense: Early.			Nonsense: Late.		
Class.	Number of scholars.	Number of correct syllables in (a) and (b).	Class.	Number of Scholars.	Number of correct syllables in (a) and (b).
O	32	220	T	29	188
N	38	282	M	41	318
K	42	304	L	40	262
J	40	297	V	38	296
	152			148	
Letters: Early.			Letters: Late.		
R	37	264	H	44	312
P	38	255	I	31	242
S	31	222	F	35	267
Q	34	344	G	20	174
	140			130	

After estimating for 147 students in the case of the nonsense test and for 130 in the case of the letter test we find the following totals:

Nonsense: Early, 1,081; late, 1,056.
Letters: Early, 997; late, 995.

The late students thus did 98 per cent. as much and 99.8 per cent. as much as the early in these two tests.

As in 3, 4, 6 and 7, so in test 5 (memory of forms), each paper was given a score corresponding to the number of pictures correctly remembered. No attention was paid here to the *position* of the pictures. The criteria of correctness were somewhat complex, certain things being determined upon as the essentials in each form and required to be present, others—*e. g.*, the number of cross lines in the fifth figure—being regarded as unessential. The totals for the different classes tested are given in Table VII.

When we estimate the work of 145 early and 145 late scholars on the basis of these results we find that those having

TABLE VII.—FORM TEST.

Early.			Late.		
Class.	Number of Scholars.	Number of correct figures.	Class.	Number of Scholars.	Number of correct figures.
T	29	130	O	30	112
M	42	182	N	36	154
L	42	183	K	40	169
V	39	168	J	40	167
Totals	152			146	

the test early remembered 628, those having it late 594. We thus get the relative amount of work of the late scholars as 94.6 per cent. of what the early scholars did.

Test 8 (counting dots) was given to only six out of these eight classes. In the case of each student record was kept of whether he counted correctly or not, and if the latter how far he deviated from the correct answer. There were fourteen dots on the chart. Consequently a paper which said '13' would be scored 'wrong, 1'; a paper which said '17' would be scored 'wrong, 3.' The test is such as requires a large number of subjects to make it valuable, and Table VIII., which presents the results obtained, is valuable mainly only because it shows that in the form test the early students were superior in general ability to the late.

TABLE VIII.—COUNTING DOTS.

Early.				Late.			
Class.	Number of Scholars.	Number of wrong answers.	Total extent of deviations.	Class.	Number of Scholars.	Number of wrong answers.	Total extent of deviations.
N	38	11	23	M	41	9	11
K	42	15	28	L	40	4	5
J	40	17	20	V	38	9	16
Totals	120	43	71		119	22	32

As the figures concerned vary so much and so regularly in favor of the *late* scholars we need not estimate carefully the differences, but simply leave the totals as a basis for the reader's judgment. The late have 81 per cent. correct answers to 64 per cent. in the early and do not make nearly so bad mistakes.

In general it may fairly be said of the results presented that they are obtained from the actual work without any complicated processes of allowances, are extremely clear in their meaning, and so calculated as to admit of very slight errors due to absences from class, estimations on per cent. basis, etc. I have gone through all the work raising the classes which had fewer students to an equality with the others by *adding* the proper percentage to their work (instead of subtracting from the work of the classes with more students, as was done in obtaining the results given), and find that it makes no difference in the general meaning of the results and no difference of over 3 per cent. in any single result. It will be remembered that in an early section of this paper I mentioned that in one class a variation in the instruction given may have lessened the work. This was in the case of T. in the early spelling. I accidentally said, "Look at each word and mark it if it is spelled wrongly," instead of saying as usual simply, "Mark this way" (showing them) "every word that isn't spelled correctly." This difference in the instructions given probably made them work a bit more slowly, and consequently a slight addition to the early spelling totals might be made, possibly enough to make ½ of 1 per cent. increase.

The percentages given would seem, on the whole, to be legitimately obtained from the individual papers, and the individual papers would seem to be fair measures of the ability of the children to do mental work. It remains to explain the details of the administration of these tests. The reader will remember that an attempt was made to eliminate the influence of the excitement due to a first visit by making the first visit correspond with an 'early' test in half the grades and with a 'late' test in the other half. This was done in the manner indicated in the following table (IX.), which shows also whether

Table IX.

	Class.	First visit. Grade.	First visit. Time.	First visit. Day of week.	Second visit. Time.	Second visit. Day of Week.	Second visit. Interval of time since first visit.
March 20.	T	Cleve. 5th.	Early A.M.	Monday.	Late A.M.	Monday.	Given same day.
	V	" 7th.	" "	"	" "	"	" " "
April 10.	K	" 6th.	" "	"	" "	"	" " "
	J	" 7th.	" "	"	" "	"	" " "
April 11.	L	" 6th.	Late P.M.	Tuesday.	Early A.M.	Wednesday.	Given the next day.
" 12.	M	" 7th.	" "	"	" "	"	" " "
April 12.	O	" 5th.	" "	Wednesday.	" "	Thursday.	" " "
" 13.	N	" 7th.	" "	"	" "	"	" " "
During last half of March and first half of April.	A	Scr. 5th.	Early A.M.	"	Late P.M.	"	" " "
	D	" 6th.	" "	"	" "	"	" " "
	B	" 5th.	Late P.M.	"	Early A.M.	"	" " "
	E	" 6th.	" "	"	" "	"	" " "
	F	" 6th.	Early A.M.	Tuesday.	Late A.M.	Tuesday.	Given a week later.
	H	" 5th.	" "	"	" "	"	" " "
	G	" 6th.	Late A.M.	"	Early A.M.	"	" " "
	q	" 5th.	" "	"	" "	"	" " "
	S	" 6th.	Early A.M.	"	Late A.M.	Monday.	Given 6 days later.
	P	" 5th.	" "	"	" "	"	" 6 " "
	Q	" 6th.	Late A.M.	"	Early A.M.	"	" 6 " "
	R	" 5th.	" "	"	" "	"	" 6 " "

each late test was late in the forenoon or afternoon session, the day of the week and the interval between tests. It will be noted that six of the twenty early tests were on Monday after the two days' rest.

Care was taken that the late tests should in every case be given after a regular day's work. Where the first visit was made late, the teacher had done her work with the class without any knowledge that there was to be a test of any sort given by anybody. Where the first visit was made early the teacher was kept in ignorance of the day when the second test was to be given and generally of the fact that there was to be any second test. Before giving the late test the experimenter made sure that a regular day's work had been done so far as the teacher's word could make that sure. Miss F. Grace Schaeffer, who conducted the Scranton tests, kept a record in every case of the school program for the day's work before a late test. These records are those of a typical school day in a city grade-school. It seems safe to say that any difference in the ability of scholars to do mental work due to two hours or more of ordinary school work without long rest, or four hours with a rest of one (Scranton) or two (Cleveland) hours inserted, would have been detected by these tests. The tests, then, measured ability to work; the process of computation of totals did not involve any appreciable error; and the work whose influence the difference between early and late results measured was normal work. Such normal work, we have seen, does not decrease the ability to work.

Apparently there is no doubt that the pupils tried as hard in the late as in the early tests, because they did as well, but it may be worth while to describe the means taken to prevent conventional school habits from influencing the work. The means were simply the conduct of the work by Miss Schaeffer (Scranton tests) and myself, the assumption of a human instead of a school-teacher's style of talk about the work, and a plain, matter-of-fact administration of the whole affair.

Some minor explanations are still to be made. In about 90 per cent. of these tests a test of the ability to add was given both

early and late (in order to get data concerning the amount of *unwillingness* to work caused by repeating a familiar task). The addition test took five minutes ordinarily. That made the total actual *working time* of the student during a test from eight to twelve minutes, seven minutes less than the total time taken up with the test. It may be objected that here the pupils have a resting time which would sufficiently prepare them for the short spurt of work. But this objection would be foolish, for they were concentrating their attention on the explanations given by the experimenter, or writing the answers to tests 3, 4, 5, 6, 7 and 8, or waiting attentively for the signal to start, during this time. They were, I should unhesitatingly say, far busier during these seven minutes than during the average recitation in school.

It may also be objected that our tests measuring the mental work done in from 1¾ to 6 minutes after a previous five minutes' work at addition do not measure real ability, but only ability to spurt, which it may be claimed is a different thing. But if the pupil can do as much in the last five of ten minutes' work at the end of school as he can in the last five of ten minutes' work at the start, and if in tests 3, 4, 5, 6, 7, 8, which came after the ten minutes, he does as well late as early, there would seem to be no reason why he should suddenly fall off in ability at the 13th or 14th minute. Nobody has ever given any evidence to show that he would. If the mental work done by school children really decreases their ability to work, such decrease ought to appear, at least to some extent, in the work done in these short-time periods. Long periods of work are very objectionable as tests of mental ability in school children, because with them you can not easily get rid of the factors of ennui, unwillingness to work, association in thought with a lesson given by a teacher, etc. I fancy that nobody save one that is obsessed by the idea that school children work too much, will think that our tests involving 15 minutes listening, adding, etc., were too short to feel the influences of mental fatigue. The fatigue must be a very mysterious thing which lets a child work 15 minutes just as well as ever and then suddenly descends upon him. Moreover, I might justly insist that, as the children had been

continuously at work for a half-hour or more before each late test, the time of commencement of the spurt be put 15 minutes before I came into the room. Then my tests should have felt the full force of fatigue. This spurt argument becomes hollow when one realizes that the tests given are not isolated things, but simply 15-minute fragments of the continuous work of the day. The spurt theorist, then, really says only that we have only proved that at any time in the school day the pupil can do as well as at any other. That is all that it is necessary to prove.

Another possible objection might be the limitation of the experiments to the schools in two cities. These cities might be exceptional in requiring less work and giving the pupils an easier day's work in general. Doubtless there are many schools where the pupils do more work in a day. The Cleveland and Scranton schools, however, probably do just as much work as the *average* city schools and very likely more, and our problem concerned the fatiguing influence of school work in general.

A fair claim then to make on the basis of the results obtained is that a regular day's work in the grammar school does not decrease the ability of the child to do mental work. I do not mean that investigation with different tests, longer tests, different schools and different ages should not be carried on with the expectation of modifying this claim. They should be. But until this is done it seems unwise to deny the claim from purely *a priori* reasons. The results obtained by previous observers we noticed at the start as conflicting one with another. In a later paper it is hoped to explain, at least to a certain extent, the factors causing these varying results, especially where they contradict those here presented. This task demands a separate treatment. For the present it will be enough to say that many of the methods (*e. g.*, Griesbach's) did not measure fatigue at all and others measured the unwillingness rather than the inability to work. So we may now turn to take a look at the bearing of our results upon the *practical* problem of school fatigue, first asking the reader to imagine from now on that he agrees with our claim, that the work done in the school session does not decrease at all the ability of the child to do mental work, that he can do as much and do it as well the last hour as the first.

This does not in any sense abolish the practical problem of school fatigue and over-pressure. The fact that the children *can* work as well does not at all mean that they *do* work as well or that measures should not be taken for their relief. It does mean that the argument for shorter hours and longer pauses, so far as *based on alleged incompetency to work under present systems*, was a false argument and its measures for relief ill-considered. We have one clear fact—that children in school ordinarily *do* not do as well in the latter part of the session. The present study has shown one more fact, that this is *not* because they are really any less able to work. To remedy the trouble by decreasing the length of hours, sessions, etc., or by giving easier lessons, would seem then to be an expensive, indirect and possibly unavailing method, for the trouble is not with their *ability*, but their desires, interests and distractions. It is not loss of mental energy, but thoughts of whispering, desires to look out of the window or at the clock, feelings of repugnance at familiar tasks, etc., which lessen the quantity and injure the quality of school work. Mere rest, mere *ease* of work might not remove these and would be at all events a clumsy remedy. The more appropriate remedy would be not to give the student less to do, but to make it *worth while* for him to work, to make the work *interesting*. Many schools in fact would find their scholars doing better work at the end of the day if they gave them *more* work, for in many classes the pupils are bored by the triviality and repetitiousness of the instruction.

No teacher then should think when she sees the scholars' work fall off each hour, "Well, it isn't my fault. If the superintendent will disregard all the laws of hygiene and insist on long work periods and short pauses, the children's ability to work will decrease just as surely as water runs down hill. They can't do but about half as much, and I should just be assisting in their mental break-down if I expected it. The laws of mental fatigue are to blame, not I."

No superintendent should think that by modelling programs after some German authority he will eliminate all of the difficulty. The mental work done is the resultant of two factors, the mental one of ability and the semi-moral one of interest,

willingness, zeal. The first we have shown to be present for the time required without any new program; the latter depends mainly not on programs or hours or rest, but on the material used for study, the methods of teaching and the personality of the teacher. There are, one might say broadly, three main factors which prevent mental work: 1st, the feeling of boredom, of repugnance, a sort of mental nausea; 2d, actual pains, headaches, etc.; 3d, real incompetency, shown in lack of associations, painfulness of all thinking, etc. The 2d and 3d are rare in school, but when present should be the charge of a school physician. The first causes, I believe, 95 per cent. of the decrease in mental work during the day in schools, and for it *good teaching is the cure*. The great burden of the child (and of many of us grown children) is not doing things that are *hard*, or that *hurt*, but doing things that are stupid and sickening and without worth to us.

I said that the fact that the children could work as well at the end as at the beginning of the school session did not abolish the problem of over-pressure. It would of course discredit if not disprove the theory that children spend so much mental energy in school work that they become nervously exhausted, in so far as that theory depends on alleged relative incompetency in children after school work. But over-pressure, so far as due to confinement, restraint, compulsion to attend to lessons possessing no zest, worry, chagrin, eye-strain, etc., would still remain.

Moreover, the reader who has in mind the conclusions of a previous article (PSY. REV., Sept., 1900), will realize that the fact that children can work as well at the end of the day as at the beginning may not at all mean that they have not worked hard or even overworked. For we saw there that the danger point might perhaps be reached without any diminution in the power to work being apparent. Personally, however, from observation of school children at work in classes, from the histories of individual cases and from my own experience, I should say that the *amount* of mental work done by pupils in grammar schools is not the cause of one-tenth of the nervous break-downs among children. The worry, misery and strain of ill-directed

effort and stupid lack of mental stimulus and healthy mental life, together with misuse of the sense-organs, seem to me to be the tap-roots of the evil. The chief responsibility for mental fatigue in the schools and for mental exhaustion in scholars falls, I should be inclined to say, not on a Creator who made our minds so that work hurt them; not on the public opinion which demands that children shall do a given amount of work but upon the unwise choice of material for study, the unwise direction of effort, the unwise inhibition of pleasurable activities, the unwise abuse of sense-organs and the unattractiveness of teacher and teaching.

I have to thank Superintendent L. E. Jones, of Cleveland, Ohio, for permission to test the children in that city, and Miss F. Grace Schaeffer, of Western Reserve University, for taking entire charge of the Scranton tests, and for her careful administration of them. To her zeal I owe very much. For the methods used, the calculations of the results and the conclusions reached I am personally responsible.

III. Fatigue of Special Functions.

The previous papers have treated the influence of mental work in general on the general ability of the mind to do work. This is the more important practical question, for it is in most situations in life possible for us to vary the sort of work we have to do and so obviate any possible bad results which might come from continued mental exertion along some one line. Theoretically, however, we may expect as much profit from a study of the influence of protracted exercise of a single mental function on the efficiency of that function. We have seen how far and how general mental work unfits the mind to work further. Let us now ask how far any particular sort of work, *e. g.*, adding, or discriminating weights, or lecturing, or composing, unfits a person to go on successfully with that particular sort of work.

It will be remembered that in the experiment quoted above it was found that three hours of the hardest work that could be devised failed to bring about enough decrease in subject T's efficiency in that particular work to outweigh the increase due

to practice. Observation of my own mental work has convinced me that in other single mental functions I can do a considerable amount of work not only without producing general inability, but also without producing any marked decrease in the efficiency of the particular function concerned. Moreover, as in the case of general mental inefficiency, we found that the reasons for a decrease in work seemed to be physical feelings, strengthened impulses to rest, etc., and other definite afferent feelings which served to bring about a conscious or unconscious inhibition of the mental activity, so I find that the reason why I do less work of some special sort after being engaged in it for a long time is not that I am unable, but exactly because I do not feel like it, *i. e.*, because I feel these afferent inhibitory feelings.

It is difficult to secure subjects who can perform the experiments needed to test this theory, that is, who will carry on a certain sort of work to the utmost of their ability, no matter whether they feel like slackening the work or not. However, my friend Dr. R. S. Woodworth kindly undertook a number of such experiments and carried them out with great care. His results seem to me to enforce the conclusion I have just stated. In them one sees no signs of any gradual impairment of the mental functions involved or indeed within the limits of the experiments (in same cases eight hours long), of any impairment at all. These results are given in detail later on.

It would therefore seem wise to accept provisionally the hypothesis that in special mental functions as well as in general mental activity decrease in efficiency is not the direct consequent and proportionate parallel of mental accomplishment without rest, but is rather due to complex causes, chief among which are feelings other than the mere feeling of incompetence.

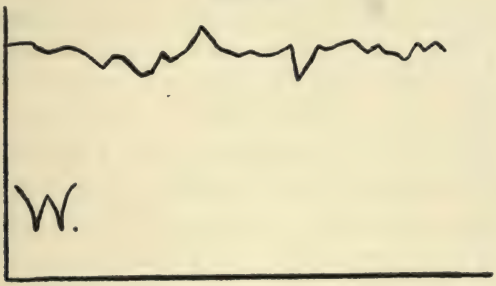
It should be remembered that this does not mean that mental fatigue has not a real existence. Not a jot or tittle of the reality of the fact is here denied; mental inability is a real thing. The view of it taken in this monograph differs from the previous views in ascribing it to different causes.

Experiment 1.

The mental work done was to mark every word containing both *e* and *t* in 151 pages of a book, each page containing about 725 words of text. At the end of each minute a signal was given by a bell and the subject made a mark denoting the point he had reached. The work continued eight hours (from 10 : 15 A M. to 6 : 20 P. M.) with only five interruptions amounting in all to less than nine minutes. The work is hard and requires close attention. It is not unlike a good deal of our ordinary intellectual labors, *e. g.*, reading, correcting proof, noticing details of forms in learning spelling, foreign languages, etc. We can measure any change in its quality by the number of words containing *e* and *t* omitted. Its quantity is of course measured well enough for our purpose by the number of lines gone over in a given time.

It was important of course to have the subject start at a point near perfection, so that practice might not enter largely. The subject had done a very large amount of such work in connection with certain other experiments some months before, and found in a preliminary test of fifty minutes that he was at a dead level (the amounts done in the several ten minutes being 238, 225, 231, 236 and 235 lines).

FIG. A.



The change, or rather lack of change, in the amount done during the eight hours' constant work is shown in Fig. A. The height of the line represents the subject's efficiency as far as concerns amount of work done. It was evident that there was no decrease.

The change in the quality of the work done has not been determined for the whole eight hours. In the first twenty-five minutes' work there were 37 omissions, *i. e.*, 6.075 omissions per hundred lines; while during the last twenty-five minutes' work there were 48 omissions, *i. e.*, 7.92 omissions per hundred lines. Thus there were 30 per cent. more omissions at the end. This does not mean that the quality was 30 per cent. worse, for six omissions in a hundred lines means six omissions out of about 200 cases. If we give the per cents for the number correctly marked we have only nine-tenths of one per cent. difference between the earliest and latest work. It is not possible to tell just how much inferiority in work is represented by an extra omission per hundred lines.

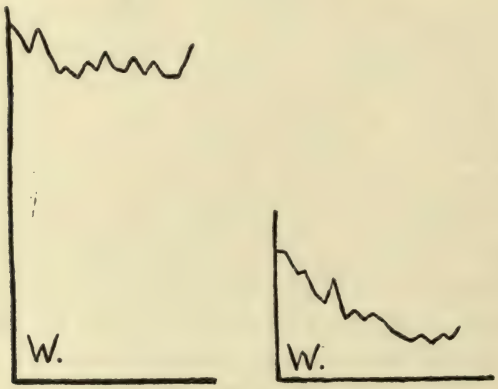
Experiment 3.

In three hours' work in estimating the areas of small parallelograms of paper the accuracy of W.'s judgment was constant for the first two hours, but fell off 7 per cent. in the last hour.

Experiment 2.

W., who had had some special preliminary drill in memorizing numbers, worked constantly from 3 to 7 P. M., memorizing

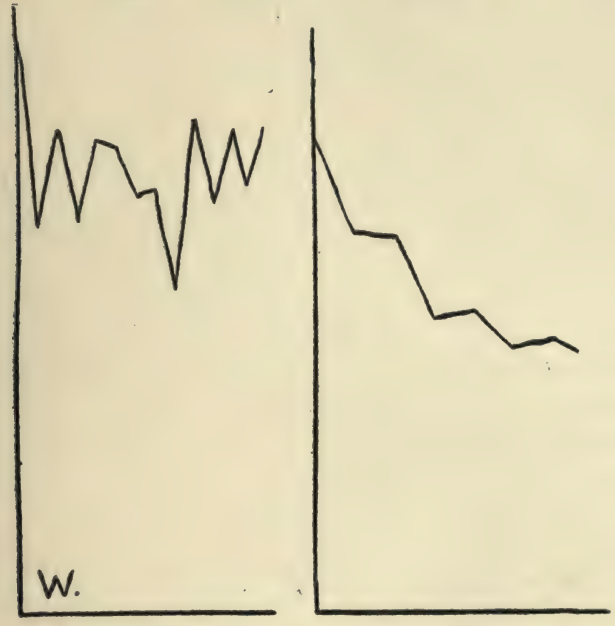
FIG. B. FIG. C.



sets of numbers written on a series of cards. Fatigue, if present, did not counterbalance the practice effort shown by Figs.

B and C, which represent the change in the quantity and quality (number of mistakes) of the work. The slight rise in the curves at the end looks like fatigue, but is probably the result of a

FIG. D. FIG. E.



growing dimness of light noted by the subject at the time of the experiment as troublesome. Apart from this there is no sign of any inability to work.

Experiment 4.

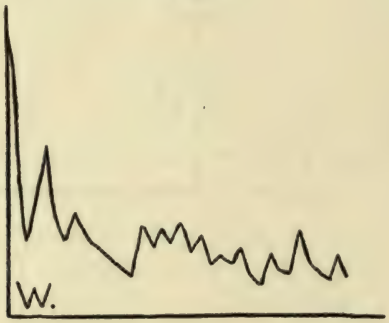
In order to have one experiment which should, as nearly as possible, duplicate an actual piece of mental work such as is frequently done in common life, W. kept an accurate record of the time taken by him in correcting each of seventy examination papers corrected in succession. He worked constantly from 3 : 30 P. M. to 10 P. M. Of course one would expect considerable assistance to be rendered by practice in this case, but if there were any decrease in ability to work it should certainly appear at the end of the series. Moreover the subject had had a great deal of previous practice in correcting papers. The

six and a half hours of work show no signs of any fatigue effect on the subject's ability. Fig. D represents the change in the amount of time taken per paper (the times for each successive five are represented by the ordinates). Fig. E represents the changes in the amount of time taken per page. Neither of these two ways of reckoning is fully correct; it was easier to look through two pages on one question than one page on each of two questions.

Experiment 5.

Another experiment was taken directly from ordinary life. W. had to go over three hundred and fifty cards on which were written titles of foreign books or articles and decide in each case whether to insert it in a certain bibliography. The work involved careful reading and translation, ability to remember the meaning of technical terms, decision as to the fitness of the article

FIG. F.



and finally decision as to how to classify it. The work took three hours with thirty-five stops of from ten to fifteen seconds (to rest the subject's eyes). The time taken to do each successive ten cards was recorded. No fatigue effect was observable. Fig. F shows the changes of the time in the course of an experiment.

IV. PHYSICAL FATIGUE AS A MEASURE OF MENTAL FATIGUE.

By using dynamometers or ergographs of various sorts we may test an individual's ability to do physical work with his muscles. It has been supposed that the amount of physical

work a person could do and the speed at which the force of a certain movement fatigued (1) are functions of the central nervous system, (2) are dependent on such general factors as are at play in mental work and mental fatigue, and that (3) therefore we may measure mental inability by physical inability. So Kemsies[1] tested school children with Mosso's ergograph, and from differences in the amount of work done at different times in the school day decided that "the school had upon certain scholars an unfavorable influence which we must ascribe to fatigue." A similar method has been used in this country in Chicago to ascertain the loss of power caused by school exercises, and the course of power during the day in certain of the schools has been mapped out.

The first of these suppositions is not decisively proved and may mean various things; the second, so far as it has a definite meaning, is mere presumption; the third, therefore, is a hypothesis to be proved, not a basis for a test of the mental fatigue due to school work. In order that dynamometric tests should have any significance for school work we should have to show how far temporary physical inability signified temporary mental inability. We have already seen how complex a thing temporary mental inability is, how many causes contribute to destroy mental efficiency. We have seen that ability to do mental work is not proportioned to the amount of work done without rest, and that consequently there are two questions, "Does the performance of mental work without rest decrease one's ability to do muscular work and in what proportion?" and "Does *real mental inability* decrease one's ability to do muscular work?"

The first question can be answered by very simple experiments. If we test people with an appropriate dynamometer before and after periods of mental work, we have only to compare the results. By paying due regard to normal variability, practice, physical work, etc., we can estimate the influence, if any, of mental work on muscular ability. The second question is, in the author's mind, too indefinite for treatment as it stands. He would prefer to turn it into a number of specific questions, such as, "Does sleepiness go with a decrease in one's muscular

[1] *Deutsche medicinische Wochenschrift*, July 2, 1896.

ability?" "Does slowness of associative-processes?" "Does inaccuracy?" "Does nervous prostration?" "Does mental irritability?" etc.

The data to be presented here concern the first question only.

The method used was to test the amount of physical work done by various people, before and after periods of mental work, the amount of which was at least approximately known. The apparatus used was Cattell's portable spring-dynamometer, which registers accurately the pressure exerted on a spring. The movements used were contractions of the first, second and third fingers of the left hand. The subjects were college students with some psychological training, an office clerk and myself. The records were taken in the morning after satisfactory sleep and in the evening after a day's class work and study or office work. The dates were so arranged that practice favored early and late tests equally. Variations due to physical work, time of day, barometric pressure, temperature, emotional condition were not excluded. The last three factors probably affected early and late tests equally, if they have any effect. The influence of the time of day could not, for practical reasons, be tested apart from physical and mental work. Very little physical work was done by the subjects of the experiments.

The subject made at each test 100, 200 or 300 contractions, one being made per second, and a rest of a minute intervening between each hundred. The general result of the experiments was to show *that mental work effected no decided decrease of physical power*. In the following figures the broken lines represent the amount of physical force exerted in early tests; the unbroken lines the amount exerted in tests after mental work.

To say that mental work does not necessarily decrease one's power to do physical work does not imply that the latter is independent of mental conditions, permanent or temporary, or that in individual cases whose mental make up was well known dynamometric tests might not be indices of various mental conditions. Among these might be certain of the phenomena of fatigue. What is asserted is that the difference between a mind before and after it has worked for six or eight hours cannot be detected by a record of physical work.

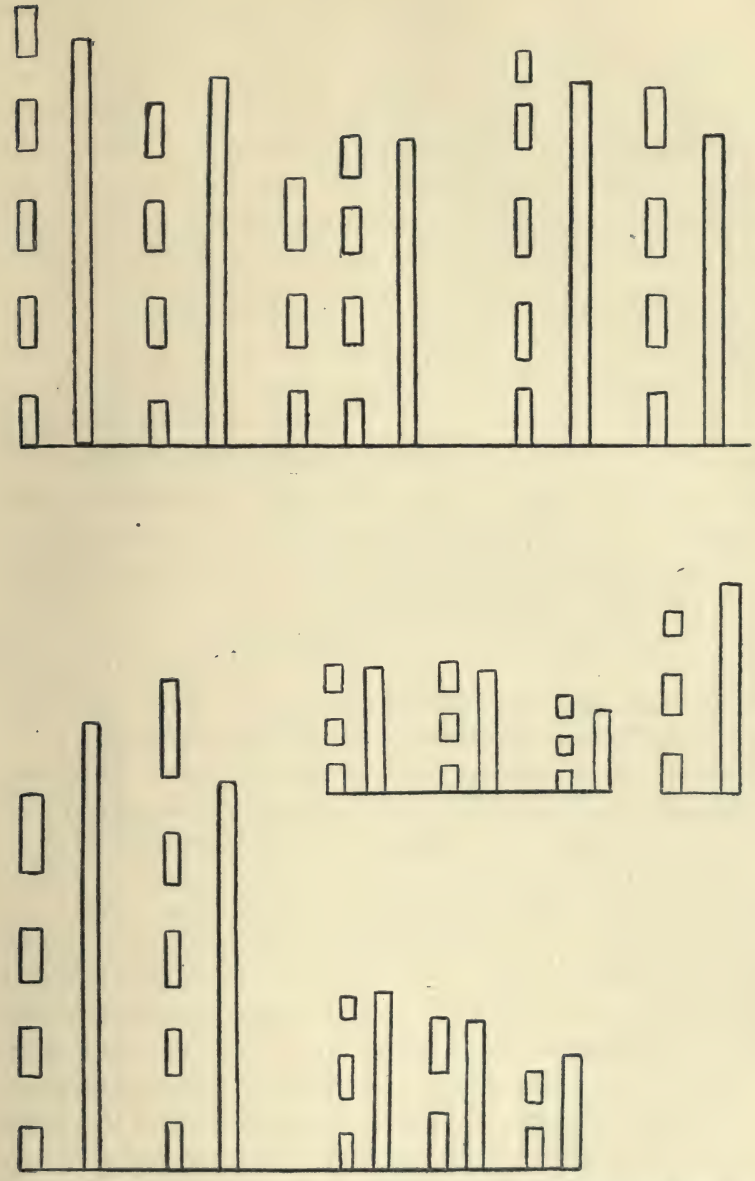


FIG. G. Graphic Representation of Physical Ability Before and After Mental Work.

WELLESLEY COLLEGE PSYCHOLOGICAL STUDIES.

An Attempted Experiment in Psychological Æsthetics.

BY PROFESSOR MARY WHITON CALKINS.

With the assistance of

HELEN BUTTRICK AND MABEL M. YOUNG.

The psychologist meets the gravest difficulties in his attempts to bring the genuinely æsthetic experience under experimental conditions, for æsthetic enjoyment cannot be secured by any fixed combination of stimuli, because it is notoriously fleeting, capricious and individual, now surprising one at unexpected turns, and again retreating coyly when most persistently wooed. The simple material, therefore, like straight and curved lines, which lends itself most readily to definite and measurable variation and has formed the material of most experiments of this sort, is peculiarly unlikely to waken the æsthetic thrill. On the other hand, the genuinely beautiful object cannot readily meet experimental requirements. For in spite of theories that there is no distinction between the merely 'pleasant' and the 'beautiful' and that æsthetic enjoyment is simply a peculiarly permanent or an unusually attentive state of pleasure, careful introspection seems clearly to support Kantian theories of æsthetics and to show that 'pleasant' and 'agreeable' differ radically and essentially from 'beautiful.' The truly æsthetic experience is indeed characterized by an almost indescribable merging of self and thing, of ego and alter, of subject and object. It follows that close introspection, the discriminating study of a momentary experience immediately upon its conclusion, is here almost impossible, since the absorption essential to the æsthetic consciousness, is not easily and promptly replaced by the scien-

tific mood. The observer is therefore in danger either of passing over the genuine æsthetic experience or else of accepting as 'æsthetic' the most lifeless and negative moments of pleasure.

For all these reasons, the investigation here described, though it has aimed to study the æsthetic consciousness by the experimental method, can not claim actually to have attained either end. It has tried to avoid the difficulty of the experiments on simple figures, by studying the enjoyment of pictures, but these pictures, though selected for their ability to arouse æsthetic feeling, may have failed of their purpose in many instances. It is indeed impossible to interpret even from their own account of it, the exact nature of the experience of the persons whom we have tested, so that plot or character-interest or some other form of un-æsthetic pleasure may have dominated their enjoyment. Moreover, the very richness and variety of the pictures, which form the material of the study, has diminished their value for strictly experimental purposes. But even when it has not sounded the depths of the genuine æsthetic experience the investigation has been of undoubted interest by bringing into prominence certain conditions of the enjoyment of pictures by young people of various ages.

The subjects of the experiment are three hundred children, equally divided among the kindergarten, the fourth primary and the highest grammar school grades of certain Massachusetts schools; and one hundred and fifty Wellesley College students, of whom one-half are freshmen and one-half seniors.

The immediate object of the experiment is a comparison of the liking of these children with that of the college students for certain typical pictures. The simple method employed is the following: Each person is tested, in a room by himself; three pictures are shown, two by two, in the same order; these two pairs are so arranged that the picture best liked, of the two which are first shown, is again compared with the third picture. Each picture represents a woman's half-figure: Picture I. is a delicately colored lithograph of Prang; it shows the head and shoulders of a pretty pink-cheeked girl who is looking downward. She wears a quaker-like cap and mantle, and the white neckerchief which is open to show her throat is fas-

tened with a bunch of violets. Picture II. is an uncolored photograph of Chantron's 'Souvenir,' shown in the Salon of 1896. The face is slightly turned away and a high light falls on the beautiful features, on the wistful upturned eyes, on the soft hair knotted in the neck, and on the delicate outlines of shoulder, arm and back, from which the garment has fallen away. The right hand, loosely folded over the left, holds a bunch of violets. These two pictures, similarly mounted, are shown side by side, with the question "which do you like better?" (To the older subjects it is explained that this question does not mean "which seems to you the better picture" or "which seems more artistic?") The picture preferred is then combined with Picture III., an *Alinari* photograph of one of Melozzo da Forli's newly photographed frescoes from the *Capitolo dei Canonici* in St. Peter's, the buoyant figure of the winged, aureoled angel who plays the violin with so serious an expression in her wide-opened eyes.

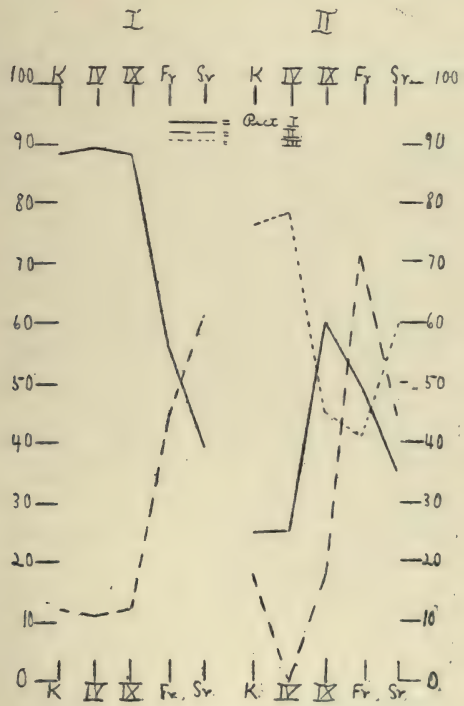
The pictures are, of course, intended to embody in a prominent way certain definite characteristics: the first is colored; the second has the beauty of form and outline, of light and shade; the third is obviously a very suggestive or associative picture, expressing distinct religious conceptions, and symbolizing spiritual experiences, besides inevitably suggesting European galleries and similar works of Italian art to some of those who were the subjects of the experiment. It is impossible, of course, to isolate any of these characteristics; all the pictures have a certain grace of outline and no one of them lacks suggestiveness. But a study of the reasons given for the choices shows that these motives predominate.

The pictures are shown when only the experimenter and the subject of the experiment are present, in order to guard as far as possible against distraction of the attention. When the three choices have been made and entered, the pictures are exhibited again in the same order and the subjects are asked to give reasons for their decisions. These choices furnish the first material for our study. The results are summarized in the table which follows, and are brought out very clearly in the accompanying charts:

TABLE I.—CHOICES.

Age of subjects.	Choice I.		Choice II.		
	Picture I.	II.	I.	II.	III.
	%	%	%	%	%
Kinderg. (90)[1]	87.8(79)[1]	12.2 (11)	25.3 (20)	18.2 (2)	75.6 (68)
Grade IV. (99)	88.9 (88)	11.1 (11)	25. (22)	———	77.8 (77)
Grade IX. (95)	88.4 (84)	11.6 (11)	59.5 (50)	18.2 (2)	45.3 (43)
Freshmen (70)	55.7 (39)	44.3 (31)	48.7 (19)	71. (22)	41.4 (29)
Seniors (75)	38.7 (29)	61.3 (46)	34.5 (10)	43.5 (20)	60. (45)

The percentages are based on the number representing the *possible choices* of a given picture. In the first choices, Pictures I. and II. were both shown to each person and either might



have been chosen each time: the percentages are therefore calculated on the number of the persons tested. In the second choice Picture III. was shown to each person and is similarly

[1] The parenthesized numerals refer, in the first column to the number of subjects; in the other columns to the number of choices.

treated, but Pictures I. and II. were repeated only when already chosen, and *either*, not *both*, compared with Picture III. The rate of preference for these pictures is based, therefore, on the number of times when they were actually shown in the second test. Picture I., for example, was 79 times preferred to Picture II., by the kindergarten children; in the second test, therefore, it was 79 times shown with Picture III. and 20 times preferred to it; and these 20 choices are 25 per cent. of the 79 possible choices.

In the first choice between the colored picture (I.) and the uncolored figure (II.) nearly nine-tenths of each class of the children, more than one-half the freshmen and nearly two-fifths of the seniors prefer the first, apparently influenced by the color. The children are evidently little affected by the charm of outline or the delicate suggestion of Chantron's picture. The result of this first choice seems therefore to emphasize the prevailing impression that children prefer bright color to any other quality in an object. The study of the second choices does not however substantiate this theory. Precisely the youngest children, those of the kindergarten and the fourth grade, least often choose the colored picture.[1] Children of fourteen, on the other hand, and college freshmen most often persist in their choice of the colored picture. The subtler charm of Picture II. has little effect except upon the college students.[2] On the other hand, da Forli's angel is, taking all in all, the greatest favorite. So far therefore, as these figures give scope for any conclusion, they point to suggestion as the most common factor of the enjoyment of pictures.

Up to this point, the material for our study, the actual choices of children and of college students, has been of a definite and

[1] The evident interest of the little children in the more novel picture must be taken into account.

[2] The apparent increase, in the second choice, of the preference of the children for Picture II., is based upon so small a number of cases that it may fairly be disregarded. The most curious feature of the Table is its indication that the liking of the Freshmen for Picture II. is more persistent than that of the Seniors. The number of these choices is not large enough to warrant any dogmatic conclusion, else we should be inclined to interpret the figures as a suggestion that the allegorizing period of the reflective young person's development is the Senior rather than the Freshmen age.

objective sort. In turning now to a consideration of the reasons assigned for these different choices it is necessary to admit at the outset the disadvantages, already mentioned, of the method. The shyness of little children often, undoubtedly, prevented any expression of reasons for their choices, and some of the older children may have hesitated to commit themselves, through a morbid sort of conscientiousness. On the other hand, it is quite likely that college students, and perhaps the older children, occasionally invented very forced reasons from a mistaken sense of scientific duty. Every effort was made to overcome these difficulties; an easy relation with the little children was established, whenever it was possible, before the pictures were shown, and the older subjects of the experiment were carefully told to record only their immediate consciousness. It is nevertheless impossible to assume that our records adequately represent the experience of the persons whom we tested.

The average number of reasons assigned (including those advanced for a third choice) is for the kindergarten children a fraction over three, for children of nine and of fourteen a number between four and five, for college freshmen and seniors five and five and one-half. An interesting general characteristic of the replies is the increase, with advancing years, of the tendency toward the negative reason, that is, the explanation of preference for one picture by dislike of another, as when the colored picture is chosen through distaste for the undraped shoulder and back of Chantron's figure. This class of negative reasons, comprising only one-fiftieth of the explanations given by the very little children, includes one-sixth of the answers of children of fourteen and more than one-fifth of the college reasons—a suggestive commentary on the growth of negative criticism.

The classification of answers to the question, 'Why do you like it?' at first takes account of cases in which the question is met by a 'Don't know' or a 'Can't tell.' Among the little children who could give no reason, the word 'Cause' played a leading rôle in such replies as ''Cause it's prettier' or 'Prettiest, 'cause I like it.' Half the kindergarten children, more than one-fourth of the nine-year-olds, and nearly one-third of the children of fourteen years give no explanation of their

choices, whereas only one-tenth of the college freshmen and an even smaller number of seniors are without this resource. This failure to offer reasons does not, of course, argue against the sincerity of the enjoyment. On the contrary, it is the very nature of the emotional, and especially of the æsthetic, consciousness not to render up an account of itself, for it is an immediate experience, which is followed, not constituted, by the reflective enumeration of its causes. The inability of children to assign reasons for their choices is no indication, therefore, of their inability to feel æsthetic pleasure.

The table which follows offers a classification of the explanations offered, excluding the non-committal answers.

TABLE II.

EXPLANATIONS.[1]

Explanation.	Age of Subjects.				
	Kind.	IV.	IX.	Freshmen.	Seniors.
Face.	1.3	8.2	8.7	13.8	10.8
Form.	1.3	6	5.9	16.1	11.1
Detail.	75.1	42.5	10.4	4.1	7
Color.	11.7	19.3	25.5	9	9.3
Expression.	.6	5.7	11.5	21.2	17.1
Suggestion.		.6	5.9	13.8	14.4
Relig.Sug'n.	7.5	4.7	4.8	.6	1.2
'Realness'		2.2	6.3	5.1	10.6
Unclassified.	2.	10.4	21.1	16.	18.

The little children most commonly make their choices because of some prominent detail of dress or of background which strikes their fancy. Three-fourths of the positive explanations of the kindergarten children and nearly one-half of those of the nine-year-olds are such as these: 'Pretty suit on and violets'; 'she has curly hair'; 'fiddle'; 'rag on her head'; 'that one has posies'; 'wings on, and playing banjo.' The flowers, sometimes distinguished as 'violets' or 'rosies,' are objects of most eager attention especially on the part of the little children; articles of dress are often named, but now more often by the sophisticated nine-year-olds; the violin and the wings win the attention of the older children chiefly; the hair

[1] This classification of 'reasons' was not pre-arranged, but suggested by the records.

is absorbing, notably to a child who supplies the personal element, and prefers the angel because it has 'curly hair like myself'; the halo is noticed and described as a 'nice head with dots.' What is observable in all this is the narrowness of the child's observation, his frequent inability to appreciate a picture as a whole.

Besides these explanations of preference by singling out some detail of the picture, the kindergarten children have practically only two other reasons for their choices. The most important of these is the liking for color, and the numerical results probably underestimate the importance of this factor, for the admired features of Picture I. were presumably often chosen for their color. Distinctly religious suggestiveness is the remaining expressed explanation of the choice of the youngest children, and is frequently named by older children, while it has no place in the college records. 'Because it's an angel,' 'it's in our church'; 'it's the Christ child's mother'; 'it's a divine picture' are examples of these reasons; and one child says of the angel: 'it's God almighty.'

The nine-year-old children, while still preëminently interested in separate details, name a greater variety of reasons for decision, noticing, for example, face and 'expression' and pose. Children of fourteen, whose fondness for Picture I. has already been remarked, realize their liking for color as the ground of one-fourth of their choices. The pleasure in 'expression' first noticed in the nine-year-olds is more often given as explanation; and under this name are grouped epithets like 'sweet,' 'joyful,' 'thoughtful,' as well as cases of the more specific statement "I like the expression." Still another reason given by these older children for liking a picture is the fact that it is 'real,' or 'human,' or 'alive,' or, as one child puts it, 'natcherel.' It is interesting to observe that this explanation is never given by the youngest children, who are too inexperienced to make a distinction between real and unreal, but that it is occasionally offered by the nine-year-olds and is most prominent in the records of the college seniors.

College students, as a whole, less often call attention to detail or to color, but more often notice the face and especially

the outline and pose of a figure; in a large number of their choices, they definitely name the expression of the face as its justification; and, finally, they show themselves distinctly susceptible to what has been called the suggestiveness of the pictures. Their records are full of such statements as 'means more,' 'more to it,' 'more soul,' "I like the conception," "it has the right feeling"; and they often name definite associations with pilgrim or puritan, or with 'something of Raphael's that I like very much.' In the same category, but among the negative reasons already discussed, may be included ten instances of objection to a picture for its incongruity with an ideal representation. Thus da Forli's angel is disliked because 'gross for the thing it is meant to represent; not a face for an angel,' while one young person goes into irreverent particulars: "the angel is fat and has a wig, and his halo looks like a door mat." Along with this increase in the influence of general suggestiveness there is a significant reduction in the number of the definitely religious suggestions, which were noticed in the experience of the little children, and this indicates the lessening importance of the religious experience with advancing years and with the multiplication of interests.

Taken together, the explanations of liking for a picture, because of its suggested characteristics, increase with years. Yet it would be unsafe to conclude that little children are unable to appreciate any save the habitual 'religious' suggestions of a picture. Judging from the actual choices, which were unhampered by timidity, by defective introspection and by awkward expression, children are very sensitive to the charm of a preeminently suggestive picture. The sweetness of the expression, or the suggestion of victory or peace which a little child finds in a picture is an experience beyond his powers of description, and he is reduced to the reply of the child who said 'I know but I can't tell'; or else, if impressed with the demands of the situation, he selects some easily named detail—like flowers, or wings, or violin—as his 'explanation,' while yet the deeper reason lurks in the fastnesses of his child soul. It would be difficult, indeed, to describe a suggestive picture in more adequate terms than those of one of the older children, a girl of thir-

teen, who said of the angel: "You can think more about it." One should, of course, guard one's self against the common assumption that this avidity for association and suggestion is a mark of æsthetic development. More often interest in the suggestiveness of a picture means the diversion of the attention from the picture itself and the breaking up of that state of absorption which is the condition of purely æsthetic pleasure. Such a comment as the following, by a college senior, shows no trace of æsthetic appreciation: "There is something symbolic, something which goes beyond the mere picture. The wings in the background—express so much." This is a good example of what William James calls 'a glow of spurious sentiment that would have fairly made old Titian sick.'

Among the 'unclassified' answers are a few which deserve a final word of comment. These are, in the first instance, cases of reference to what may be called the technical merit of the picture, to 'clear cut lines,' 'better finish,' 'execution' or 'drawing.' Most of these occur among the older children and the college students, but a few nine-year-olds seem to have been trained to observe these characteristics, and some of their comments, like "stands out more" (of picture II.) and 'not finished in appearance' (of Picture III.) show a genuine observation which far surpasses the vague indefiniteness of such replies as 'more artistic' or 'better art' which are found among the records of the older subjects of the experiment.

The distinctions between the freshman and the senior records are apparent in the table. The differences are too unimportant and the figures too few to demand comment: the greater sensitiveness of the seniors to the suggestion of a picture and their livelier pleasure in its naturalness or realness are the only noticeable distinctions. A very careful comparison of the girls' reasons with those of the boys' gives no support to the theory of a fundamental psychic difference between masculine and feminine point of view. Only two differences are characteristic of all three ages: the girls more often notice the face and the expression in the picture. This distinction seems to be most simply explained by the fact that the less active life of the girl, even in her earliest years, has fostered her interest in dolls and

indoor-plays and picture books, and so has trained her to observe faces and expressions.

The comparison of reasons no longer on the basis of the different ages, but on that of the different pictures, justifies the selection of these particular pictures for the study, since it shows that Picture I. is most often chosen through liking for the color, that Picture II., more frequently than the rest, is preferred for the attitude and outline, the expression and the face; and that Picture III., setting aside the reasons of 'detail' which are given by the children in their frequent selection of it, is most often chosen for its suggestiveness.

The table follows:

TABLE III.

EXPLANATIONS.[1]

Explanation.	With Pictures.			Explanation.	With Pictures.		
	I.	II.	III.		I.	II.	III.
Face.	8.3	14.3	9.7	Suggestion.	4.6	7.9	13.7
Form.	7.8	11.1	7.9	Relig. Sug.	——	.3	7.7
Detail.	24	3.6	27.2	'Realness.'	5	10.4	5.1
Color.	28	11.1	2.8	Miscel.	11.1	19.4	14.8
Expression.	10.8	21.5	10.6				

The more general results may be summarized, even at some risk of repetition, lest they disappear in the thicket of percentages. Chief among them is the conclusion that it is impossible to draw absolute distinctions between child and adult, so as to say definitely of this or that picture, "no child will like it," and of this or that motive "no child will be susceptible to it." On the other hand, leaving out of account the preferences of the kindergarten children, there is no type of choice which is not represented among children as among adults; and the common opinion that children, especially little children, are sure to prefer a colored to an uncolored picture is sharply contradicted by our records which show that children are far more impressed by what is called the 'suggestion' of a picture, when this is

[1] The figures suggest a comment on the fact that Picture I. is chosen nearly as often for some detail as for color. As has already been suggested, this special feature of the picture may have been liked for its color; and furthermore, the little children who so often chose Picture I. were frequently too shy to give a reason, although they were very likely influenced by the color.

not of the very subtle sort. What distinguishes the æsthetic judgment of children from that of adults is rather the habit of dwelling on the parts to the neglect of the whole of a composition, and the tendency to fix the attention on the details of figure or of background, and to enjoy a picture through interest in its unessential features. One practical outcome of this study is therefore the suggestion that training in art should aim directly to widen the scope of attention and to stimulate the capacity, never attained by the uncultured person, of seeing parts in relation to each other and in subordination to the whole.

AN ILLUSION OF LENGTH.

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Two years ago, while studying the æsthetics of geometrical forms, we required the students to produce 'double squares' by direct eye estimation. The forms were produced by means of an adjustable frame and were to be made twice as long in the horizontal direction as in the vertical. Not only was the illusion of the vertical counteracted, but there appeared to be a tendency to make the horizontal distance too short.[1]

We repeated the same test with two hundred school-children,[2] ten boys and ten girls of each age from six to fifteen inclusive, and found that this illusion of length is much stronger for children than for adults, and that it decreases with the increasing age of the children. Thus, the children of six made the double square 28% too short, the 7's 24%, the 8's 25%, the 9's 14%, the 10's 10%, the 11's 11%, the 12's 11%, the 13's 10%, the 14's 8% and the 15's 9% too short. These figures seem fabulous, especially when it is remembered that they represent the amount by which the well-known illusion of the vertical has been outweighed. The illusion of the vertical was measured in the same series and found not to vary noticeably with the age of the children.

Continuing the study of this illusion in persons who knew little or nothing about the nature of the illusion, we made a series of measurements with university students in introductory psychology, before the subject of illusions had been discussed in class. Twenty-nine men and thirty-four women volunteered to be tested. The so-called unconscious method was followed

[1] "Forty-eight made the horizontal line too short by an average of 15 mm. (mean variation, 8 mm.), and fourteen made it too long by an average of 10 mm. (mean variation, 6 mm.). On the whole the horizontal distance was made 4½ per cent. too short." *Univ. of Iowa Stud. in Psych.*, 1899, II., 18.

[2] *Op. cit.*, p. 33.

in so far as it was possible. Most of the observers, however, surmised that some illusion was involved in the test, and the conscious or subconscious reaction to such suspicion often entered into their judgments. The records of two men were discarded because these men asserted that they had attempted to eliminate the illusion of the vertical. The observers may evidently be divided into two classes, namely, those who introduce some correction for supposed illusions and those who do not. But as such division is relative and cannot be made satisfactorily either upon introspective testimony or upon inspection of the results, we group the records together and state some of the conclusions which may be drawn from the averages of the results.

The figures to be studied were cut out of cardboard and then placed on a neutral background upon a large drawing board, which was so placed that the figures were 50 cm. from the eyes and at right angles to the line of regard. Some of the figures were drawn on the background, as will be explained. Every effort was made to eliminate disturbing influences from color, brightness, limiting lines and spaces, movements in the adjustment of the cards and other known sources of error. The forms of figures may be grouped into five classes, as is shown in the accompanying table. They are: (A) parallelograms, (B) a vertical and a horizontal line, (C) and (E) two horizontal lines of different length, and (D) unequal horizontal distances limited by points.

In the A-series rectangular forms were to be produced as follows: A1, a double square, double in the horizontal direction; A2, a double square, double in the vertical direction; A3, a half square, the standard vertical; and A4, a square, the standard vertical. The standard for all was the side of a square, 114 mm. A card 114 mm. wide and about 275 mm. long was used, and the observer was required to produce the respective forms by placing a similar card over such portion of it that the remaining part would constitute the required form. The figures in the B-series are parallel to those of the A-series and consist of only two of the adjacent lines from each form of the parallelograms. They were to be produced as follows: B1, the horizontal line twice as long as the vertical; B2, the ver-

tical line twice as long as the horizontal; B3, the horizontal line half as long as the vertical; and B4, the horizontal line equal to the vertical standard. The variable line was 275 mm. long, and the observer covered such portion of it with a card that the remaining part of it stood in the proper ratio to the standard line. In Case C the two lines lie in the same direction, horizontal, and the measurement was made as in the B-series. The variable line was placed 10 mm. lower than the standard in order to eliminate the motive to connect them. In Case D the standard distance (horizontal) was given by two points. The observer was required to place a third point so far to the right of these that the distance between the second and the third points appeared to be twice as great as the distance between the first and the second. In Case E the figure is the same as in Case C, but the observer was required to select the limiting point before applying the limiting card.

Form.												
Case	A1	A2	A3	A4	B1	B2	B3	B4	C	D	E
St.	228	228	57	114	228	228	57	114	228	228	228
	X d	X d	X d	X d	X d	X d	X d	X d	X d	X d	X d
M	222 9	213 10	62 3	115 3	226 7	211 6	64 3	119 2	225 8	227 11	217 7
W	222 5	214 5	63 3	116 3	223 7	216 5	63 2	120 4	226 7	227 9	216 9
Ave.	222 7	214 7	63 3	115 3	222 7	214 5	63 2	120 3	226 7	227 10	217 8
% E.	−3 3	−6 3	+11 5	+1 3	−2 3	−6 3	+11 4	+5 3	−1 3	0 4	−5 4

Form, the shape and the position of the figures and forms.

Case, the designation of each case in this report.

St., the required length of the measured line or distance. The unit of measurement is the millimeter.

X, the average of the results of the estimates: (M) by men, (W) by women, and (Ave.) by all together.

d, the averages of the mean variations.

%E, the differences between the estimate and the standard, stated in percentages of the required lengths. This is the illusion. The mean variation is also reduced to percentages.

The number of trials is two for each person upon each point in Cases A, B and D, and ten in Cases C and E. The double fatigue order was observed.

The final averages of the results may be seen in the accompanying table. Case A1 represents the original form of the illusion. The double square is made 3% too short. This

indicates that the illusion of length exceeds the illusion of the vertical by that amount, because the two illusions stand in direct opposition in this position of the figure. If only the illusion of the vertical had been present the double square would have been made too long. Case A2 shows the effect of the coöperation of the two illusions. Here the double square is made 6% too short. We have found, in a large number of other experiments, that the two illusions in coöperation produce an illusion equal to the sum of the two illusions acting singly; still that is not always to be expected, because the effect of one illusion partly satisfies the motive of the other. Case A3 was introduced as a check upon the method of making the comparison and the measurement. In form this figure is identical with A2, but it differs from it in size, and was produced by varying the shorter dimension. This half-square was made 11% too wide. In all our experiments it is found that when the vertical distance is varied there is a stronger tendency to correct the known illusion of the vertical than when the horizontal distance is varied. This is because more attention is called to the presence of that illusion by the former method. That law does not apply to the illusion of length, because the observers had no means of knowing that such an illusion existed. As will be shown later, the difference in size does not account for the difference in the illusion for the half-square and the double square. Case A4 contains only the illusion of the vertical. The horizontal distance is made 1% too long, but that is not a representative figure. Some four hundred measurements of other students gave an average of 5% for the illusion of the vertical. It is difficult to say how far this tendency to correct the illusion of the vertical enters into the estimation of the double square in the present experiments. It is probable that it increases the difference between the two illusions in A1, and that it decreases the resultant of the two in A2 and A3.

We conclude from the study of the A-series, then: (1) that the illusion of length obtains for both the horizontal and the vertical positions of the double square, (2) that it is stronger than the illusion of the vertical, and (3) that the tendency to make unconscious corrections for an illusion is greatest when the image of the illusion is clearest in the mind.

The B-series was so arranged as to determine whether the illusion is a linear illusion or whether it is peculiar to the comparison of the two dimensions of rectangular areas. The relations of the dimensions of a parallelogram may be estimated: (1) by comparing a vertical margin with a horizontal margin, (2) by comparing the length of imaginary lines that cross at the center of the figure and (3) by judging by the total impression with reference to the areal content without distinctly selecting representative lines. The first method is the easiest and the most accurate, and was followed by nearly all our observers. When the second method is employed the illusion is increased on account of the increased vagueness of the parts to be compared. The third method is the most variable and probably entails the greatest illusion. Children in their quick and spontaneous judgments follow this method, as a rule, and this accounts partly for the strength of the illusion with them. When the first method is employed we have essentially the same conditions of comparison in the A-series as in the B-series. The average of the results for B1, B2 and B3 are equal respectively to the results of A1, A2 and A3. B4 shows a normal illusion of the vertical.

From the comparison of the results of these two series of tests, we conclude that the gross illusion of length in surfaces is essentially a linear illusion.

Is this linear illusion contingent upon the difference in the direction of the two lines compared? In Case C both lines are placed in the same direction, and the illusion of the vertical is eliminated. All the results for lines lying in the same direction show that the illusion is present in such lines, but is very much diminished by the elimination of the difference in direction.

Case D was introduced in order to determine whether the illusion of distances in the same direction is contingent upon the presence of lines. The results are affirmative; there appears to be no illusion for the distances between the dots.

Case E was introduced in order to determine whether excessive eye movements constitute one of the motives for the illusion. In Cases A, B and C the comparison is naturally made by bisecting the long line visually, to determine whether one half of

the long line is equal to the length of the short line. When one half of the line is regarded the eye wanders beyond the middle point into the other half, on account of the guiding power of the line and the absence of a definite objective limit. This overrunning of the point of regard ought to produce an overestimation of the half that is regarded, and thus an overestimation of the whole line. Case E is like Case C in every respect except that there is an additional motive for excessive eye movements in Case E; the eye is allowed to wander to the right beyond the limit to be selected. The results show that the introduction of this additional motive for excessive eye movements produces a marked increase in the illusion. Since this new eye-movement motive which is present in the bisection of a line is of the same nature as the additional one which increases the illusion in Case E, we conclude that it is one of the primary causes of the illusion of length. We may refer to this as the first motive.[1]

Contrast is a second primary motive which may be present and coöperate with the first motive. It is an application of the principle of relativity—a long line is compared with a short line, and, according to this principle, the long line should appear to be longer and the short line shorter than it really is. With young children this contrast is undoubtedly the strongest motive, because children have strong tendencies to overestimate differences. It has been demonstrated that illusions that have physiological causes, *e. g.*, the illusion of the vertical and the Müller-Lyer illusion, do not vary in a marked manner with mental development; but the illusions of judgment, *e. g.*, the illusions of contrast and illusions of time, vary enormously with mental development.[2] Therefore it is probable that, if both the physiological and the psychical motives were present in those of these tests that were made upon children, the variation with age (mental development) is due chiefly to variation in the second motive, namely, contrast.

[1] Incidentally we have here a crucial test of the eye-movement theory of the Müller-Lyer illusion. Cases C and E may be considered the simplest forms of the Müller-Lyer figure. All the motives demanded by current theories of this illusion, except one, are eliminated in these figures. The eye-movement motive is the only one present, and the illusion is present and varies with this motive.

[2] Seashore, *op. cit.*, pp. 33–35, 83, 84.

These two motives appear in the comparison of lines that lie in the same plane and direction, but when the two lines lie in different directions or are parts of parallelograms, there appears in addition to these a third primary motive. It is a well-known fact that if a square and a double square appear side by side the latter will appear to be the narrower. This is not due to the contiguity of the two figures, for the dimensions of the double square do not appear to change when the square is removed.[1] Some would ascribe it to the principle of relativity, the two sides of the same figure being compared with each other. But it may be accounted for better by Wundt's theory of eye movements, *i. e.*, there is a stronger tendency to move the eyes in the direction of the longer lines than in the direction of the shorter.[2] This theory is in harmony with the most acceptable theory of the visual perception of space and is well supported by the fact that the total illusion of length is so much greater for parallelograms and lines at right angles to each other than for the lines that lie in the same plane and direction.[3] The results tend to show that this third primary motive, which has no connection with the first eye-movement motive, is, for adults, the strongest motive in the figures that contain it.

To determine the validity of the method employed, we made parallel measurements by the method of selection. The observers were allowed to select the required proportions from series of cards. The results obtained by this method support those that were obtained by the method of production as described above.

We also made a series of measurements by the method of selection to determine whether the ratio of 2 : 1, employed above, is peculiarly favorable for the production of the illusion. The observers were required to estimate the length of cards in terms

[1] The theory that the apparent narrowing of the double square is due to an illusion of perspective is disproved by the fact that the figure does not appear to change when the square with which it has been compared is removed, and by the fact that the length of the figure is overestimated, instead of underestimated.

[2] Wundt, *Die geometrisch-optischen Täuschungen*, Abh. d. math.-phys. Cl. d. k. Sachs. Ges. d. Wiss., XXIV., 1899, 158.

[3] A part of this difference may be accounted for by the fact that the placing of the lines in different directions increases the difficulty of comparing and thus gives fuller sway to the illusory motives that are present.

of their standard width. There appears to be a gradual increase in the illusion from its inception near the square up to the ratio of 2½ : 1, which was the largest ratio tried. The illusion is not relatively stronger for the ratio of 2 : 1 than for the adjacent ratios.

Does this illusion vary systematically with the size of the figures? The averages of the results for four observers who made eighty trials each upon each of four sizes of cards of the A2 type above, by the method of production, are as follows: standard (length in the vertical direction) 76 mm., illusion —7%; standard 114 mm., illusion —8%; standard 228 mm., illusion —7%; and standard 456 mm., illusion —8%. The cards were in all cases 75 cm. from the eyes. There is therefore no constant tendency for the combined illusions to vary with the size of the cards. But it has been held that the illusion of the vertical alone increases with the size of the object. We therefore measured the illusion of the vertical for the corresponding sizes of squares. The same four observers made eighty trials each for each size of card, by the method of production, with the following average results: standard 57 mm., illusion —3%; standard 114 mm., illusion —2%; standard 228 mm., illusion —4%; and standard 456 mm., illusion —3%. Thus there is no constant tendency for the illusion of the vertical to vary with the size of the object, and therefore the same is proved for the illusion of length.

DISCUSSION AND REPORTS.

THE DISSIMILARITY IN FUNCTION OF THE RODS AND THE CONES OF THE RETINA.

What I have proposed to call the normal night-blindness of the fovea (PSYCH. REV., 1895), in parallelism with what has long been known as the (abnormal) night-blindness which is a symptom of *Retinitis pigmentosa*, a disease of the pigment epithelium, one of whose effects is to prevent the formation of the rod pigment and consequently of that surrogate vision which the normal eye acquires in a faint illumination, has not been admitted by all observers to be an actual phenomenon. Tschermak and Sherman, in particular, have denied its existence, while that has been as vigorously affirmed by v. Kries and his assistants. A new series of experiments has now been devoted to the question by v. Kries and Nagel[1] (the latter a dichromate). They point out in the first place that the retinal area tested by other observers was altogether too large; even though it did not exceed the size of the rodless region, it was certainly fully equal to it, and there is no possibility of securing such absolute fixation as would be necessary to confine a retinal image for any appreciable time upon a given exact portion of the retina [even if it was not for the fact that focal fixation in a faint light is any way an exceedingly difficult matter on account of the fact that we have for practical reasons carefully learned to avoid it]. They also desired to meet the objection of Tschermak to their former work (*Ztsch. f. Psychol.*, XII., 1)—that they had not secured a sufficiently long adaptation, and moreover to determine more carefully than they had done before the exact extent of the non-adaptable region.

The dichromate, and especially the deuteranope ('green-blind'), have great superiority over the non-defective individual as experimenters in subjects of this nature. A difference in brightness is hard to distinguish with accuracy when it is overlaid with difference of color also. But for the dichromate, who sees the entire warm end of the spectrum in yellow, it is easy to make an equation between red rays and

[1] 'Weitere Mittheilungen über die functionelle Sonderstellung des Netzhautcentrums.' J. von Kries u. W. A. Nagel. *Ztsch. f. Psychol. u. Physiol. d. Sinnesorgane*, XXIII., 161–186. 1900.

green rays (to give them their normal names) such that the two fields compared are absolutely indistinguishable. The reinforced vision of twilight, which exists hardly at all for the extreme ends of the spectrum, will then exhibit itself in an excessive brightening of the 'green' field (the difference may amount to a hundredfold), and its failure in the fovea will be evidenced by the fact that for that area the fields remain alike. Instead of exhibiting the two fields side by side, the authors found it much better to use the now familiar 'spot' method——one field is seen through a small hole in the other, and complete likeness of the two is recognized by the disappearance of the hole. (Acuity of vision is so great in the center that the boundary line does not become absolutely invisible throughout its whole extent as it does in the periphery, but there is, nevertheless, perfect certainty as to the equality of the fields.) The size of the hole was in the first instance sometimes ¼° and sometimes ½°. Tried by this method, and with all imaginable subsidiary precautions, it was found that in the exact center of the retina the Purkinje phenomenon does not occur; when the adapted eye is first opened, the spot (which has first been made equally bright with the surrounding 'red' field for the daylight vision of the observer) is seen to shine out with an intense brightness, but as soon as the (minute, black) fixation-point in its center is secured the difference between spot and field absolutely vanishes. There was also no falling off in the saturation of the 'green' spot when viewed centrally, as there would have been if the achromatic twilight vision had overlaid it.

Experiments were also made by v. Kries himself (that is, with normal vision) by means of comparing two colorless mixtures of different light-ray composition—that mixture which contains green shines out brilliantly in comparison with the other after adaptation (the extended Purkinje phenomenon), and here also the phenomenon was found to be wholly wanting in the center. The same result was obtained, again, when an equation was established, for v. Kries, between the spectral lights red and green on the one hand, and yellow on the other. In one and all of these methods, it will be noticed, the two things to be compared differed not at all in quality, and there was nothing therefore to confuse the judgment of equal intensity; they are much to be preferred to that of the simple estimation of the relative brightness of two adjacent heterochrome fields, as red and blue. [Blue, it should be remarked in passing, is not the color to be chosen in making these comparisons with red. The 'Purkinje phenomenon' is usually stated to consist in the intensification of blue in a faint light

(all mention being omitted of the fact that the blue becomes less saturated at the same time, and that the real phenomenon is a *whitening* of the blue), but this is surely only an intermediate stage, corresponding to that in which the rods are filled with the visual *yellow*, an absorbent for the blue portion of the spectrum. After adaptation is complete, it is green that has become the most intensified, not blue. The first stage may be called by its historical name, the Purkinje phenomenon; the final stage may better be known (for the sake of distinction) as the *extended Purkinje phenomenon*. The gradual change from one stage to the other is exhibited in a diagram in Tonn's paper (*Ztsch. f. Psychol. u. Physiol. d. Sinnesorgane*, VIII., 280), and the fact that the cause of the change is probably to be found in a mixture of two absorbing media gradually changing in relative amount (the *Sehroth* and the *Sehgelb*) is indicated, for the mathematician, in the fact that the several curves have a nearly common point of intersection. The visual yellow is a less marked stage in the regeneration of the visual purple than in its degeneration, when the substances are studied objectively; it would be interesting to know if there is any difference between the subjective brilliancy of blue, according as it is seen in a condition of semi-adaptation following upon the daylight condition or upon the night condition of the eye. This simple experiment has not, I believe, been tried. This intermediate state of vision might well be called twilight-vision, and to the state of complete adaptation the name night-vision might be applied; our ancestors have already made for us the proverb, ' in the *night*, all cats are gray.' Discussion is so voluminous just now in regard to all these new ideas that it is of extreme importance that a good and sufficient phraseology should be adopted as one goes along.

V. Kries and Nagel also made careful experiments to determine the exact size of the adaptationless area by varying the apparent size of the spot by moving it forwards and backwards; it was readily found that the apparent size at which it just fails to undergo a brightening on its outer border is, for the two eyes of a single individual which were tested, 1.40° or 1.8° horizontally, and 1.35° vertically; they also found that at a slightly greater distance from the center there was a sudden sharp access of brightening. They suggest that this latter distance is that at which the arrangement of one cone in a complete circle of rods begins, and that a few scattered rods may occur up to the region of no adaptation, in spite of the fact that Koster gives the rodless region as 2° in diameter. v. Kries thinks it possible that the rod-pigment when outside of the rods, when just formed out of the substance of the pigment epithelium, may be effective upon the adjoining cones. This

suggestion is very lacking in probability, but it is a satisfaction to find that v. Kries begins to attribute to the rod-pigment its evident rôle of being the source of night-vision; he has been speaking even quite lately as if the rods had no daylight-vision at all, and as if the achromatic vision of the normal periphery were necessarily to be assigned to the cones. But this is to overlook the fact, which stares one in the eyes, that the most striking feature of the whole situation is the coincidence between a gradually on-coming re-inforcement of vision, most effective in *green* light, and an equally gradual regeneration of the *green*-absorbing substance in the rods.

But there is a still more marked coincidence between function and apparatus adapted to meeting it, which goes very far toward confirming this view that the visual purple acts as an absorbent medium for an insufficiently strong light. All vertebrates except fishes have a visual purple of exactly the same absorption spectrum; in the eye of the fish the maximum absorption is farther toward the blue (Köttgen and Abelsdorff). The difference is perceptible to the naked eye—the color in fishes is plainly more bluish. Now in what respect do fishes, on the one hand, and mammals, amphibians and birds, on the other, differ as regards their faint-light vision? Plainly in this: the dark recesses that other animals have to enter are the depths of forests, the shadows of overhanging plants and grasses. But fishes find their darkness at great depths in the water, and water has its maximum absorption in the yellow (Spring: Bul. Acad. roy. de Belg., 1896 (3), XXXI., 251). The blue absorption stuff of fishes is, therefore, just what is adapted to the retaining of the yellower light of the depths of the ocean. There is another curious feature in the visual apparatus of certain fishes—the retinal tapetum, a white surface of guanin, in the upper half only of the retina, whose evident function is to give preponderating reflection to the light which comes from regions down below, while the too bright light of the external upper half of the visual field is still moderated by the absorption of the black pigment of epithelium and choroid. This is an instance of an absolutely special provision in the eye of the fish for the darkness of ocean depths which is quite analogous to the distinctive color of their visual purple, and hence lends probability to the view that that too is a differentiation in the interest of adaptation to life under special conditions.

It is a very great satisfaction to have established, at last, beyond question, the fact of the failure of night-vision in the fovea (meaning by night-vision that form of vision which is acquired after the rod-

pigment—the visual red or the visual yellow—has had time for regeneration, and which is recognized subjectively by the great change which takes place in the relative brightness of the different portions of the spectrum). Experiments on this subject which are made by estimating the brightness of two fields of different color are not to be compared in value (even if they had not been done with too large a field, and by an inadequate method) with those in which the observer is merely required to detect the disappearance of a spot in a surrounding field with which it is absolutely identical both in brightness and color. This necessary condition can be secured in two ways: (1) By the method of the *achromatic color equation*, as it may be called, for the normal eye; in this method the distracting effect of the color-aspect of the fields to be compared is obviated by making each consist of a pair of complementary colors mixed in the right proportion to produce gray, and therefore indistinguishable for sensation when equally bright, though physically of very different constitution. The field which contains green will be the one to brighten up in a faint light, for twilight-vision (being due, as we may suppose, to the absorption into the rods of green light by means of the visual purple) is predominantly vision for the green portion of the spectrum. (2) The same result, that of offering to the observer a difference in brightness only though the ether-wave constitution of the two fields compared, is very different, and can be had with no difficulty throughout either half of the spectrum in the case of the partially color-blind. It was by this method of the achromatic color-equation that Hering's hypothesis of the specific brightening power of the colors (or Hering's complete color theory, if he had not quickly modified his views—*vide* Tschermak) was overthrown: if twilight vision was a new form of vision, mediated by the visual purple of the rods, and for that reason achromatic, and if its changed brightness values were completely accounted for by the character of the visual purple absorption, then there is no room left for a 'specific brightening power of the colors.' Hering's theory is now, therefore, again in its original extraordinary position, in which spectral red, *e. g.*, which most observers regard as being absolutely saturated, owes all its brightness to a black-white process (whose presence at all is a purely gratuitous assumption), and is not affected in its brightness in any degree by the voluminousness of the photochemical process of an exactly similar sort which underlies the sensation of red.

With this definitive result of v. Kries is removed every difficulty that may have existed in granting (what is a starting point for my theory of color vision) that the rods and cones play a different part in the

retinal economy—viz. (to give it accurate expression) that *the rods are the organs for nothing but achromatic vision, and that color is mediated by the cones only.* This result may be regarded, therefore, from now on as a distinct acquisition to our knowledge of the sense for light and color.

This view would have been rendered still more certain, perhaps, if it had turned out that all the cases of congenital total color-blindness show also total blindness in the fovea. The first case of that kind to be discovered (the defect does not exhibit itself without some device for making it apparent, on account of the fact that the patient learns carefully to avoid fixating with a spot in the retina with which he can see nothing) was that of the totally color-blind boy in König's laboratory upon whom I applied (1894) the method [1] of showing that some one of a *group of spots* is sure to disappear if such defect exists, when the effort is made to see them all at once—a method which I had just devised in order to render readily perceptible the total lack of the Purkinje vision in the fovea of the normal eye, which had just unexpectedly revealed itself during some experiment which I was carrying out in a dark room in Professor König's laboratory (PSYCHOL. REV., II., 143). Since then there has been much discussion as to whether this total blindness in the fovea of the achromate is the general rule. Uhthoff first announced that in a patient of his it did not exist, but since then he has found (using a modification of my method—the person tested endeavors to hold one spot fixated in the exact center of a ring of others) that this same patient can in fact not see anything with the center of the retina. Hess and Hering, Pflüger of Berne, and v. Hippel, have all contributed cases in which this foveal blindness is lacking (they have not yet however used the last described method). But the extreme interest with which the final settlement of this question was regarded is now set at rest by means of the case of Raehlmann (described in the September number of this REVIEW). This case has been known of, as a matter of fact, for many years—that is, though foveal blindness was not expressly looked for, not having been hitherto suspected, it was known that Frau Professor R. had perfect visual acuity, which is incompatible with the fovea being thrown out of function—but it has hitherto failed, through some accident, to be accorded its proper logical weight in the discussion. What is placed beyond dispute by it is that con-

[1] I had already predicted, as a deduction from my theory, that the vision of the congenitally totally color-blind would be found to be vision with the rods only.

genital achromasy *may be* a defect of the cortical centers; this has long been known to be very frequently the sole cause in cases of acquired achromasy (see Förster's case, among many others, *Arch. f. Ophth.*, 36 (194), and since retina and cortical cells are both essential links in the chain of causation in question, and since it is quite certain that there are separate centers for color-vision, there is every reason to suppose that mal-development, as well as disease, may sometimes cause the latter to fall out of function. But, on the other hand, the undeniable cases in which total lack of color-vision is accompanied by total blindness of the fovea, even though they are not exhaustive of all cases, still point as strongly as before to the cones of the retina as being, in these instances, the source of the defect. What is rendered certain by the case of Raehlmann is that there is nothing forced in the referring of cases of a different sort from these to a different seat of the physiological lesion, namely, the cortex.

There is now, therefore, nothing that stands in the way of believing that: (1) *The rods are the organ for nothing but achromatic vision;* (2) *Color is mediated by the cones only;* as I maintained, following upon Max Schultze and Parinaud, in 1892.

C. Ladd Franklin.

THE ILLUSION OF DEFLECTED THREADS.

Professor Pierce has reported in the September number of this Review some new and interesting observations on a thread illusion which I described in May, 1898. He has also presented an explanation of the illusion which he regards as simpler and more direct than that originally offered. One almost despairs of ever finding an explanation of any optical illusion that shall command universal acceptance. But it is doubtless the duty of those who are engaged in the investigation of these illusions to compare notes as often and as fully as possible in order to reach some clearness as to the grounds of their differences, even if they find it impossible to accept each other's positions.

The illusion under discussion has as its essential feature the apparent turning of certain parts of two threads so that these parts do not seem to be continuations of the threads to which they belong, but seem to extend away from the threads into the third dimension. The threads which form this illusion lie at different depths in front of the observer, and the upper one passes over the lower one at an acute angle. The points at which the two threads seem to cross are, under these condi-

tions, different for the two eyes. It is the middle part of each thread, or the part between the two crossing points, that is apparently deflected. In some cases, and it should be noted that it is in some cases only, the image of the middle part of the upper thread fuses, not with the other image from the same thread, but with one of the images from the lower thread. That is, instead of the two images from one thread holding together throughout their whole course, they fuse only outside the monocular crossing points. In the middle parts, that is, between the monocular crossing points, they break away from each other and then they may fuse, as was pointed out in the original article, with images from the other thread, even though that other thread is at a different level. It may also be added that this unusual form of fusion may take place even when the two threads are of different colors, so as to give the images very decidedly different characters.

Now the essential difference between Professor Pierce's explanation and mine is, that I lay greatest stress on the *separation* of an image from its true companion, while Professor Pierce lays greatest emphasis on the *fusion* of the images after separation. He assumes that separation of the images is easily possible, and that the moment a new mode of fusion offers itself as a possibility it may be adopted as easily as any other possible mode of fusion. Professor Pierce finds in the presence of a possible new mode of fusion a sufficient explanation of why images which come from a single thread and have been fused through a part of their course, should in the middle part of their course be separated from each other and fused with images from an entirely different thread. In taking this view he discusses only cases in which the point of fixation is chosen half way between the two threads, thus rendering easy the assumption that the images from both threads were double from the outset. His problem, as he sees it, is therefore merely one of finding possible modes of fusion.

The objections that offer themselves to Professor Pierce's view are as follows: First, the deflection often takes place in only one image of one thread. The first case of the illusion discussed in the original paper[1] (and I have confirmed its validity since by a large number of observations with many subjects) is that in which the point of fixation is on one of the threads (not half way between the two threads). When this point on one thread is fixated, the two images of the fixated thread fuse with each other as usual. One of the images, and usually only one of the images, of the other thread will be deflected. These statements can be verified by using colored threads, so that the different

[1] PSYCHOLOGICAL REVIEW, Vol. V., p. 288.

images can be distinguished. This fact shows clearly that deflection is prior to any recombination, and, so far from being dependent on the recombination, is the source of the conditions which make a new and unusual mode of combination possible.

The second objection is this, the mode of fusion to which Professor Pierce refers in his explanation is not the only mode of fusion possible with the images that are given. There is a perfectly regular mode of fusion between the images belonging to the same thread which would result in a perception of each thread as a continuous whole lying at a different level from the other thread. This usual mode of fusion takes place outside of the monocular crossing points. It takes place there so readily and completely that it is very difficult to perceive the double images. Now if the two modes of fusion which are possible, namely, the usual form of continuous fusion and the one that actually takes place, were both equally possible, and if one is chosen to the exclusion of the other, particularly if that one is, to say the least, unusual; then the explanation is not complete until we have been told why this particular choice is made. The statement that a certain choice is made is merely the descriptive preliminary to the more fundamental question of why it was made. As a mere possibility the chances are no more in favor of one mode of fusion than of the other. But in the illusion in hand complete normal, or usual, fusion never takes place in portions of the thread lying between the monocular crossing points. Something has happened to prevent it. What is that something? Certainly it is not enough to say that another mode of fusion has presented itself as possible. As a matter of fact, another mode of fusion has appeared which is not merely possible, but *necessary*. And this necessity Professor Pierce has not explained.

Finally, to deal with two particular points which Professor Pierce dwells upon with great emphasis. The first is that the apparent deflection appears even when the monocular crossing points are covered up. The conditions that result from this covering up of the monocular crossing points are essentially different from those which appear in the ordinary illusion. In the ordinary case of the illusion there is a mixture of what may be called monocular and binocular modes of interpretation. The result is that we see both the separate threads at different depths and also the oblique, deflected threads. When the monocular crossing points are covered, the middle parts of the threads are cut off from the end parts. The result is that the binocular mode of interpretation is very much weakened. So much weakened, indeed,

that *one can never see both the deflections and the threads in their real positions, but only the deflections.* Now the seeing of the deflections *only*, means that the two images that enter a single eye are treated exactly as if they came from a single plane. They might, for example, have been lines drawn on a single plane card. But this treatment of images which really come from different depths as if they came from a single plane, is what the original article attempted to point out as the essential characteristic of monocular vision in general, and of this part of the threads in particular. In covering up the monocular crossing points and isolating the middle sectors of the threads, Professor Pierce has, therefore, shown what becomes of the illusion when the binocular factors are cut out of the original conditions and the monocular factors alone are left. He has thus contributed a very interesting and striking confirmation of the general position that images which fall in a single eye are treated exactly as if they came from a single plane. What Professor Pierce has failed to see is, that the particular case of the illusion which he emphasizes is a particular and limited case, not the original form.

The second point with which I wish to deal is one on which I have no doubt that Professor Pierce will be in fullest agreement with me, but one which it is perhaps well to bring out for the sake of the general discussion of the whole subject of illusions. Professor Pierce writes in one place, 'nothing but the universal laws of tri-dimensional vision are here in operation.' And again he writes, "In view of these various considerations I cannot refrain from the conclusion that Professor Judd's illusion presents no new visual principle * * * I am convinced that we have before us in this illusion only a particularly interesting case of what is eternally happening whenever we open our eyes to see."

The article describing the illusion was very misleading if it gave the impression that its author supposed that the facts presented led to the discovery of any new principle. I take it that the value of any illusion is to be found in the fact that the old 'universal laws of tri-dimensional vision' are absolutely reliable and unchangeable. What we try to do is to see how unusual appearances can be reduced to these same universal principles. Now in the illusion under discussion the appearance is unusual, while the principle of vision may be safely assumed to be the same as usual. The unusual appearance must be due to an unusual combination of conditions. That unusual combination of conditions is looked for in the fact that a part of the field is seen chiefly as one eye would look at it without the help of

the other, while the rest of the field is seen by both eyes in joint action, just as it usually is seen under ordinary conditions.

When Professor Pierce says of the result that it is 'what is eternally happening whenever we open our eyes to see,' I should be in most hearty accord with his statement if it referred to only the general principles of vision involved. Certainly visual perception must here, as elsewhere, follow the long and fully established mode of this form of mental activity. But if Professor Pierce means that under the ordinary conditions of visual perception, that is, under the conditions which are constantly recurring when we open our eyes to see, images from one object may fuse for a certain distance and then suddenly break off to fuse with other images in the field of vision and finally come back to the original mode of fusion—if, in other words, he means that the conditions of this particular thread deflections are everywhere common, then I shall have to take exception to his conclusions. The distinction between uniformity of principles and invariability of typical conditions is worth keeping clearly in mind—for, if I mistake not, the value of the whole study of illusions depends on this distinction.

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THE SPACE-THRESHOLD BY THE PSEUDOSCOPIC METHOD.

Recently my attention was called to an article by Professor Stratton which appeared in your issue of November, 1898, and which I found contained a serious error. Professor Stratton came to the remarkable conclusion that binocular relief was still perceptible even when the images which produced it had a separateness of less than 24″, or in other words that our sense of binocular perspective was more delicate than our visual acuity. It seems to me that if this is true it must overthrow our ideas as to the rods and cones being the ultimate visual units. A glance, however, at his method of reaching this result reveals the source of his error. He has compared pseudoscopic with stereoscopic vision without allowing for the fact that this is practically equivalent to doubling his interocular distance. His results therefore should be multiplied by two, which gives 48″, and this, according to Professor Stratton's own statement, closely agrees with the results obtained by other observers.

F. H. VERHOEFF.

The objection which Dr. Verhoeff raises to my results with the pseudoscope, in so far as he bases it on their *a priori* improbability, will perhaps be met to some extent by my paper in the September number of this REVIEW, which has appeared since his communication was written. By an independent method it is there shown that spatial discriminations by the eye may go as low as 7″ of arc, and thus be considerably less than the suspicious 24″. And I have tried to show that even this smaller value may still be harmonized with the assumption that the rods and cones are the ultimate elements of vision.

As to the computations in the pseudoscope paper, I feel that Dr. Verhoeff's criticism is due to a misunderstanding of the essential features of the experiment, for which some obscurity in my report is doubtless to blame. His objection would seem to me valid if the depth-effect noticeable at 580 meters had been *produced* by the rapid alternation of pseudoscopic and stereoscopic vision. For at the very moment of transition there might well be a summation of the eye-movements peculiar to each of these kinds of sight, and consequently a movement double that which was present in the normal stereoscopic view. But in the original experiments I myself never got the impression that the alternation itself called the observed depth-effect into existence. The pseudoscope contributed merely a 'flat' standard, by comparison with which we might more readily convince ourselves that what we saw in the normal scene was truly stereoscopic relief, and not something due merely to the vivid coloring or the light and shade.

In view of Dr. Verhoeff's objection, however, I have repeated the experiment and feel assured that this original impression was correct. That the plastic effect in the distance is not a product of the transition is clear from the fact that it does not fade away when the transition itself is past and gone, but persists indefinitely in the normal binocular view. Even a day after the pseudoscope has been laid aside the relief is unmistakably present at 580 meters and more. And what is perhaps still more convincing, this same stereoscopic appearance, one can recall, is noticeable even before the pseudoscope is put to the eyes, although the observer then lacks the subjective assurance that his observation is correct, which comes the moment later when he sees the same scene deprived of this stereoscopic character.

The depth-effect at 580 meters is thus evidently produced by conditions lying wholly within our normal binocular vision, and the rôle of the pseudoscope in my experiment was not to add to the angular disparity of the two images and so aid in producing this effect, but merely to facilitate the recognition of the plastic character present

in the normal view all the while. The computation of the angular disparity which gives this stereoscopic relief should consequently be based on the actual interocular distance, and not on double this distance, as Dr. Verhoeff maintains.

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A MAGNET REGISTRATION KEY.

The writer has for several years noticed a special difficulty in the use of break-circuit keys in reaction-time experiments, namely, that there was a tendency, especially among those who were just learning or who had not had recent training, to press the button so hard while they were waiting for the signals as to open the circuit, and so frustrate experimentation. This fault recurs also from time to time with subjects in fairly good training, especially if they are using the shorter modes of reaction. In some cases it has even seemed necessary to have them keep the hand just above the button and not in contact with it till the habit of light pressure was established.

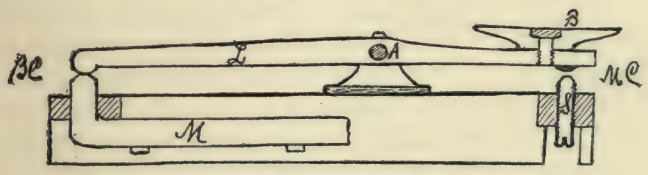
The reactors themselves will not readily believe that they are making the trouble by pressing too hard on the key and show surprise when it is demonstrated that they do. A break in some other part of the circuit or a premature reaction is frequently erroneously suspected. The trouble was particularly pronounced—in this case with trained subjects—in some experiments in which a break key was used as a shunt, perhaps because, even if the circuit was not actually broken, the pressure on the key increased the resistance in it so far as to send the current through the chronoscope.

The explanation of the fact is probably this: The lever of the usual form of key is held against the fixed contact piece by a spring. When the button is pressed down, the spring offers a continuously increasing resistance to the movement, and the contacts may separate slightly without giving any distinct feeling of the transition from contact to separation, by which the subject may be guided. There is, perhaps, also a tendency especially in the short or motor forms of reaction to secure a definite sensation from the pressure on the button and to exert such a pressure on it that the smallest possible addition will break the circuit and so make the reaction shorter. In the absence of a distinct warning signal that the contacts are separating, the pressure on the button can easily become too great and may be much greater than the subject himself is aware. It is of course not impos-

sible that the pressure may in some cases be of a still more involuntary and automatic character.

This trouble has not been observed in cases in which the registration key was used to close the circuit except when the distance between the make-circuit contacts was made very small, so that only a slight movement could be made and the shock of contact was inappreciable under the conditions of the experiment. If, on the other hand, the distance between the contacts is large a considerable error is introduced, as is doubtless also done by varying within wide limits the amount of pressure of the spring or hand on the lever.

Accordingly, it occurred to the writer that if a key were constructed that would hold the contacts together sharply and give a marked warning when they begin to separate, the difficulties and inaccuracies above mentioned might be much reduced if not wholly removed. As is well known, the contact of a piece of iron or steel with a magnet gives exactly the conditions desired, and the instrument figured below in a longitudinal section through the center, in which the contact surfaces are held together by magnetism instead of by a spring, serves the purpose in view in a thoroughly satisfactory manner.



Magnet Registration Key.

The lever L, which is about seven and one-half inches long, is made of soft iron and rests at BC on the small permanent magnet M. It is so cut and balanced that the end AL is a little heavier than AB, so that however great the movement of the lever the forward end of it will have a slight tendency to fall toward the magnet, by which it will be caught and held fast on contact. To avoid friction or binding, the axis A works loosely in the bearings, and its contact with the bearings is not depended upon for the transmission of the current, but this is secured by a couple of spirals of wire from the axis to the metal supports. The button B is made large so that the ends of three or four fingers may be placed upon it. The contact for breaking circuit is at BC. In the instrument made by the writer, the surfaces of contact were left plain iron or steel but they might be improved by covering with thin sheets of platinum, if a stronger magnet were used, and yet a sufficiently distinct warning be given on the separation of the contacts.

The make-circuit contact is at *MC*. The top of the screw *L* may be brought to within quite a small fraction of a millimeter of the contact projection of the lever and yet the shock of separation from the magnet be distinctly felt. A rather close approximation to the accuracy of a break-circuit contact may thus be obtained.

The strength of the permanent magnet is a matter of importance. The one used in this case is made from a one-fourth-inch steel rod, five inches long, and seems fairly suitable.

When the key is used for break-circuit purposes, a soft-rubber cap may be placed over the top of the screw *L*, and a movement of two or three millimeters be allowed so as to make the sensations very distinct without injuring the make-contact surfaces.

The sensation of breaking contact with the magnet is so definite that the beginner quickly learns what force to employ, and any mistake made by such a person or a more experienced reactor is known at once. By breaking contact occasionally between experiments to get an idea of the maximum pressure permissible and desirable a rather uniform standard may be maintained throughout.

The making of the sensations and innervations connected with the registration of reactions in the way described more definite may prove to be a matter of some importance as well as convenience in psychological experimentation. A special use may also be found for the key in the observation of important occasional events, astronomical or other, in which great accuracy is desired, but in which lack of training or excitement might interfere with the result.

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PSYCHOLOGICAL LITERATURE.

The Spiritual Life, Studies in the Science of Religion. By GEORGE A. COE, Professor of Philosophy in Northwestern University. New York, Eaton & Mains; Cincinnati, Curts & Jennings. 1900. Pp. 279.

This volume is an important representative of the recent movement to carry the methods of science into the study of the facts of the religious life. It is an empirical study, in which a fund of distinctly psychological data furnishes the groundwork for bringing into relation and into perspective many apparently isolated phenomena of religion. The analysis and synthesis of spiritual facts among themselves and in their connections with facts of non-religious character are done with discrimination and skill.

Perhaps the chief interest of the volume and that which especially makes it a valuable contribution to the psychology of religion is the study of temperament, both as a factor in the variety of religious experiences and in the determination of the peculiar types of religious expression. Personal records were obtained from 77 persons whose experiences had either contained striking religious transformations or an unfulfilled expectation in regard to them. These persons were questioned also as to likes and dislikes, laughter and weeping, anger and its effects, habits of introspection, moods, promptness in decisions, ideals, the effects of excitement, habits with respect to physical activity, etc. In addition to reliance upon this self-analysis, Professor Coe made an exhaustive study in various ways of the individual cases. Objective observation of the written records and also of the habits and peculiarities of the persons studied was brought to bear. Of especial value was a series of tests from the use of hypnotism and suggestion.

The cases formed two main groups: persons who expected a religious transformation and experienced it, and those who expected but did not experience it. In regard to each of these groups Professor Coe raised the question, in the first place, as to whether intellect, sensibility or will was predominant. He ascertained that "where expec-

tation is satisfied, there sensibility is distinctly predominant; but where expectation is disappointed, there intellect is just as distinctly predominant." "A second interesting result is that those whose expectation is satisfied belong almost exclusively to the slow-intense and prompt-weak varieties, the temperaments approaching most nearly those traditionally known as the melancholic and sanguine. On the other hand, those whose expectation is disappointed belong more largely to the prompt-intense variety, or the choleric temperament."

In studying the relation of these experiences to mental and motor automatisms, it was found that 65 per cent. of those who had had a striking transformation had also exhibited automatic phenomena, while only 8 per cent. of those who expected a transformation in vain had had either an hallucination or a motor automatism. In other words, "the mechanism of striking religious transformations is the same as the mechanism of our automatic mental processes."

The most significant and the most unequivocal results came from the study of the relative susceptibility of the two groups of persons to the influence of hypnotic suggestion, and their action under it. The question was as to the parts played by external suggestion and by auto-suggestion upon these two classes of subjects. Among those whose expectation of a transformation was gratified, 13 out of 14 were 'passive,' *i. e.*, they were suggestible, but exhibited little spontaneity under the operator's influence. Of 12 persons whose expectation was disappointed, all but one may be said to have belonged to the 'spontaneous' type. Professor Coe points out in a forcible way the bearing of these facts upon the phenomena of revival meetings, trances, visions, miracles, etc.

The chapter on 'Divine Healing' is farther removed from empirical data, but is carefully analytical and convincing. The last chapter, 'A Study of Spirituality,' follows up the thread of the influence of temperament in religion. "The conclusion upon which all these diverse lines of investigation converge is that organized Christianity in general, Protestant as well as Catholic, places insufficient value upon the more masculine, active or practical qualities of goodness; or, to speak in directly psychological terms, that the forms of religious life natural to the choleric temperament are habitually discounted in favor of those natural to the sanguine and melancholic temperaments, particularly the latter."

The style of the volume is clear and attractive. The book is a valuable contribution to both science and religion.

EDWIN D. STARBUCK.

The Psychology of Conjuring Deceptions. By NORMAN TRIPLETT. Am. Jour. of Psych., XI., 4, pp. 439–510. July, 1900.

Mr. Triplett presents a very comprehensive survey of the various aspects of conjuring which contribute data of psychological interest; and these are exceptionally diverse and instructive. Viewed genetically, the instinct to deceive, and later the pleasure in the exercise of deception, may be considered as expressions of helpful developmental traits; guile and cunning, craft and strategy contribute to the fitness that survives. Even mimicry involves the deception of the animal that preys; although it is obviously not a psychological reaction of the preyed upon. From this to death-feigning, to purposive strategy and the war of wits, to the recorded cases of shirking among domesticated animals, to the unconventional lies of children and the conventional ones of society, there is an irregular but mutually illuminating series of reactions in which deception plays an important though variable part. Conjuring as an art likewise opens out an historical background, in which appear the magic rites of savages, the ceremonies of primitive religions, the beginnings of physical science, the increasing control over the resources and energies of nature. The dual aspect of deception, the fact that there must be deceiver and deceived, demands attention to the processes by which the latter is misled equally with the devices by which the conjurer mystifies. Indeed the more elaborate forms of conjuring become far more psychological than physical, depend far more for their success upon the induction and suggestion of the proper mental attitude and inferences on the part of the spectators, than upon mere dexterity or mechanical ingenuity; and herein lies the great value of this material to the student of the psychology of deception.

Mr. Triplett's study places this material at the disposal of the psychologist in far more convenient and well-arranged form than has hitherto been done. His elaborate list of classes of conjuring tricks, while inevitably involving cross-classifications and very disparate rubrics, furnishes a satisfactory guide to the more essential principles which these tricks involve. A dominant principle, most frequently illustrated, is the kinship of conjuring to suggestion; for it is the suggestion of things not done quite as much as the concealment of those that are done that determines the success of modern conjuring. Of the conjurer as of the diplomat it is true, as Mr. Triplett cites, that "he says what he does not do, he does not do what he says, and what he actually does he takes particular care not to say anything about." Extreme suggestibility leads the way to hallucination. When a ball

was tossed in the air twice, but on the third time was allowed to drop behind a desk (the hands completing the motion as though the ball had really been tossed), 40 per cent. of the school boys and 60 per cent. of the girls who witnessed the test saw the ball go up and disappear at the third (suggested) throw. From this simple suggestion to the more complex ones that dominate stage illusions, or that invest the spiritualist's séances with mysteries of his own creation, the transition is psychologically close. So in the end the study of deception involves the study of suggestibility, of credulity, of mental contagion.

Interesting as may be the general trend of the argument thus outlined, the interest is markedly increased when applied to the analysis of concrete examples. The unexpected and ingenious application and combination of principles arouses admiration when the trick is revealed, and profound mystification when it is performed. For such detailed illustrations of the psychology of conjuring, Mr. Triplett's able study will serve as a useful compilation and exposition.

J. J.

Beiträge zur Entstehungsgeschichte der neueren Ästhetik. By Wilhelm Kuntz. A doctor's dissertation at the University of Wurzburg.

Dr. Kuntz divides the development of modern æsthetics into three periods. The first of these is the period of preparation in the psychology of the Middle Ages as well as in the conceptions of nature and art characteristic of that time. Elements akin to the concept of sublimity are to be found in stoicism and still more in early Christianity. The concepts of the fantastic and the humorous were not found among the ancients.

The allegorical character of mediæval art was due to emphasis on the ethical and hostility to pagan art. In the psychology of this period Dr. Kuntz believes that he finds the first traces of a theoretical foundation of the symbolic. The passages referred to in the works of Augustine and Thomas Aquinas give slight ground for his conclusion. But St. Augustine does contribute to the development of the concept of unity in the manifold in his recognition of the æsthetic value of the ugly.

The second period is that of the Rennaissance. Dr. Kuntz traces the rise of classicism in art and criticism as typified in the dramatic canon of the unities.

Part III. deals with the rebellion against classicism and the beginnings of the return to the natural. It is characterized by the grow-

ing importance of the concept of fancy and the union of the ethical and the æsthetic, and by opposition to the dramatic canon.

Dr. Kuntz would find a dialectical development from the æsthetic point of view among the ancients through the ethical point of view in the Middle Ages to a union of the two in the present. One might argue that a selection of authorities differing from that of Dr. Kuntz would give a very different conclusion.

The dissertation as a whole is a very good summary of the earlier development of modern æsthetics.

A. LEROY JONES.

THE ANALYSIS OF SENSATIONS.

Berträge zur Analyse der Gesichtswahrnehmungen. Von F. S. SCHUMANN. Zeitsch. f. Psych. u. Phys. d. Sinnesorg., Bd. 23, Heft 1 u. 2, pp. 1–32.

This is the first of a series of articles that Professor Schumann proposes to devote to the results of an introspective analysis of the more complex mental processes. In the introduction the author justifies the undertaking, that scarcely needs justification, with the statement that the exploration of the field of sensation which must form the foundation of all experimental psychology, even though many of the results fail to find a permanent place in the science, has now been carried very far, and it is time to begin to extend the investigations to the processes that lie behind the sense organ and which must eventually form a large part of the superstructure of the discipline. Many suggestive remarks are made on the place of introspection in psychology and the safeguards that must be thrown about its use if errors are to be avoided.

The author chooses for his first contribution a study of the effects of the attention upon simple patterns and through that an investigation of the nature of visual form. The investigation begins by noting how from simple patterns it is possible to pick out a series of different figures by emphasizing first one feature then another by the attention. For instance, a number of straight lines drawn parallel can be grouped in twos and seen as a series of laths with spaces between as easily as they can be seen as single lines. Lines of print are grouped in threes when counted, and each three becomes a unit, perceptual as well as conceptual.

Similarly a mass of squares can be seen in many different ways by the arbitrary action of the attention, as groups of twos, fours or sixteens. The black squares may be made dominant or the white lines

that separate them, and the white lines can be made to take on any pattern at will; but whatever pattern they assume, that for the moment holds the attention and constitutes the object that is seen.

A similar line of inquiry is carried out with reference to the perception of simple forms. So, for instance, it is shown that a square differs from an oblong because in the square each line claims consciousness in approximately equal degree, while in the rectangle the two longer sides claim the attention to the exclusion of the shorter.

Again, a square with a diagonal vertical makes a different impression from a square on its base because in the one case the vertical diagonal divides the area into two parts and two adjoining sides are united in perception, while in the other case the opposite parallel sides claim the attention together. That this is the explanation can be seen more clearly from the fact that attending to one corner of the square upon the base makes the diagonal drawn from that point dominant in consciousness, unites adjacent sides in a single whole for consciousness and makes the figure identical for perception to the square with its diagonal vertical. Numerous other simple geometrical figures were shown to owe their peculiar form to the way in which their separate elements are conjoined in attention. These observations seem amply sufficient to justify Schumann's theoretical conclusion that visual geometrical form is for perception not as Meinong and Ehrenfels have insisted a new *quale* added to the spatial ultimates, position, direction and extent, but a new way of perceiving or arranging these spatial elements, the recognition of a new unity among them. The form depends upon the method in which the elements are grouped by the attention. What is meant by this term unity is made clearer by the statement of its three concrete marks.

1. Within a group there is immediate recognition of unities of varying degrees, just as there is immediate recognition of the varying degrees of unity that result from tone fusion. A series of ten circles is more like a homogeneous straight line if seen as a single whole than if perceived as five groups of twos.

2. The effect of a group upon the attention is more unified if the group is seen as a whole at a glance, than if each of the elements is considered for itself, and first one and then the other catches the attention.

3. Each group has a different effect in recalling ideas from every other group.

While these marks serve to point out rather than to prove the nature of the unity, they are sufficient for the attentive observer, and furnish

as clear a demonstration as can be expected with reference to an ultimate element of experience.

W. B. PILLSBURY.

AFTER-IMAGES.

Beobachtungen über den Verlauf centraler und extramacularer negativen Nachbilder. ANTON WALTHER. Pflüger's Archiv, 77, 53, 1900.

By means ot an arrangement of holes in screens, round spots of any color or degree of brightness (and surrounded by any sort of a background) could be presented to the eye for central and for excentric vision at the same time, and thus the course of the after-image under these two conditions could be conveniently compared. Many interesting points of difference were found to obtain. It seems from these observations, as from so many others, that the process for color in the visual organ is something distinct from that for the achromatic sensation; after the colored after-image has faded out there is a colorless residue which persists for some time longer. The excitation, whatever it may be, which is the basis of the after-image is distinctly different for red and green on the one hand, and for blue and yellow on the other; in the case of red and green, the double-process [if we may use this term for the persistent (positive) image and its negative successor taken together] runs its course in much the shorter time; thus if the color presented be a yellow-green, the purple, which is the after-color to the green in the mixture, will soon lose its red and the after-image will seem much too blue. So if the original color is violet or blue-green (peacock), its after-image will be over-yellow. (This doubtless accounts for the fact, frequently stated, that an after-image is not always seen to be strictly complementary in color to its primary.)

If one gazes steadily at a colored surface it becomes quickly whitish on account of the retinal impression being overlaid by the already developing after-image, and if the illumination be diminished the opposite color will even appear in place of the actual one, though the eyes are open; here also, when the hues are not the primary ones, an excessive amount of blue or of yellow will appear in the final result. In all these instances the greater duration of the blue and the yellow is still more marked in the extra-macular region than in the center. (This may be expressed by saying that there is poor red-green vision in the periphery, and that in the after-image it gets poorer still.)

If the original impression be central, the after-image (it is the negative after-image that is in question throughout this paper) does not set in at once—it undergoes a gradual development and then a gradual fading out. But in the case of the excentric image this preliminary pause and gradual development could not be noticed—the image appears at once, and with full saturation; at this time, therefore, the excentric image is both brighter and more saturated than the central one, but on the other hand it fades out much sooner. It is known that the after-image goes through periods of brightening and darkening, and even of vanishing and returning; this feature also is more marked in the periphery.[1] Walther confirms the observation of Hering that several images can be started up one after another, and can then be seen to run their different courses at the same time. This completely eliminates, of course, all the effects of a mistaken judgment. This investigation adds another confirmation to the theory regarding the disjunction of function of the rods and cones: it has already been said that there is a colorless constituent of the total sensation which runs a different course from that which furnishes the color; but if the image of the spot is strictly limited to the rodless region of the retina (2° in diameter—an area of 2 mm. across at a distance of 25–30 cm. from the eye), as may be done by the method of holding the object, centrally fixated, within a ring of other spots, this difference completely disappears (*i. e.*, the dissociated gray constituent of the image no longer exists). It will be seen that this investigation furnishes a very interesting addition to our knowledge of the after-image.

C. Ladd Franklin.

Die Erschienungen bei kurzer Reizung des Sehorgans. By Herbert Munk. Zeitsch. f. Psych. u. Phys. d. Sinnesorgane, XXIII., 60–100.

The article is a study of after-images, especially emphasizing the important and hitherto quite neglected influence of the background. The separation of the sensation from its after-image by the so-called latent period, as well as the break between the phases of the positive after-image, is found to become less marked as the contrast between the stimulus and its ground decreases until they entirely disappear; and the after-image appears as a unitary process, continuous with the sensation. Not only the breaks in the process, but the modifications of the positive after-image as it gradually disappears are shown to be the effect of the contrast between the main stimulus and its ground, which

[1] But see results of Munk, to be noticed later on this point.

becomes more pronounced during the development of the after-image than at its beginning.

The theoretical discussion is a cautious attempt to explain the development of the contrast in the after-image through the diffusion of light, on the schema of G. E. Müller's general theory.

The author's experiments with colored stimuli, while making no pretensions to exhaustiveness, are entirely congruent with the foregoing. The complementary phase of the positive after-image is found to disappear entirely when the colored stimulus has for its ground a neutral gray of like brilliancy.

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THE EAR, HEARING AND ORIENTATION.

Ohrlabyrinth, Raumsinn und Orientirung. E. VON CYON. Pflüger's Archives der Physiologie, Vol. LXXIX., Nos. 5 and 6, January 31, 1900, pp. 211–302.

This elaborate and interesting research on the bodily organs correlate with the sense of space and the faculty of orientation is rich both as a report of experiments and as a summary of the opinions of others who have investigated these rather important subjects. The article is divided into seven divisions with the following titles: (1) introduction; (2) observations and research on Japanese dancing-mice; (3) rotation experiments on children, apes and tortoises; (4) separate observations and research on the orientation of carrier pigeons; (5) geotropism, heretofore the 'static sense'; (6) conclusions as to the sense of space, and the normal stimulus of the labyrinth of the ear; and (7) historical account of the sixth sense.

The experiments of Professor von Cyon were made on a peculiar variety of mice, on lampreys, children, apes, tortoises, and on carrier pigeons, but chiefly on the mice. The interest of these animals lies in the fact that they have practically only one, namely the 'uppermost' of the three semicircular canals which most of the higher animals possess [see a description by B. Rawitz in *Archiv für Physiologie*, 1899, Nos. 3 and 4, pp. 236–243]. When they walk their direction is oblique, and when they run it is always in a circle. By 'labyrinth,' the author means the semicircular canals with their ampullæ and the sacculus, and does not include the cochlea, etc., as is often included in the labyrinth by anatomists. The otocysts of molluscs, crustaceans and other low forms are considered the homologues of these

canals. Space cannot well be afforded for description of the numerous and interesting experiments. Those made on the Japanese dancing-mice are in seven divisions, discussing respectively their peculiar movements, the cause of these movements, the hearing-organ of the mice, the direction of the mice's movements, a 'blindfolding-research' on the mice, their power of equilibrium and of coördination, and a rotation-research made upon the animals.

According to this investigator the labyrinth in general is the organ by whose functioning the animal orients itself in space, especially as regards the maintenance of equilibrium. The labyrinth from which this power of orientation arises sends both inhibitions and stimulations to the cerebral centers which have to do with regulating the innervation of the eyes, the head and the back. These currents, by coming from both halves of the body at the same time, tend largely to keep them in equilibrium. This maintaining of the equilibrium, whether when standing normally or when lying or in any other position than these, seems to the author 'only a special case of orientation in space.' All of these depend on the intervention of the same nerve-centers; all occur in space, and all are connected with changes in the relations which the whole body or any of its parts bear to the three dimensions of this space. The difference which has commonly been made between static and dynamic equilibrium or orientation and the need of searching for separate organs for these are therefore wholly needless. The maintenance of equilibrium is in part due to the noises which are continually falling on the ear, that of the heart-beat for example being constant should those of extra-bodily origin fail. It is not difficult for von Cyon to carry this matter still further and to suppose that the noises act as continual stimuli possibly on other nerves as well as of the labyrinth, namely, on those connected with the short hairs of the macula acustica in the sacculi. The acoustic hairs of the ampullæ are stimulated especially through tone-generating vibrations of the air and help in this way in the estimation of the direction of sounds and so in the production of spatial sensations. The stimuli which bring about orientation in space act only temporarily, while those which allow of equilibrium are continuous.

Orientation at a distance, a faculty common to very many sorts of animals and notably to the carrier-pigeon, seems to von Cyon to depend not on reflex and instinctive processes, but on those that are 'premeditated and conscious.' In this faculty the labyrinth is only accessory, sight and 'a special direction-sense located in the mucous membrane of the nose and perhaps also in that of the frontal sinuses'

(which are connected with the nose by the infundibulum, in the case of man) being also concerned; he considers that the sense of smell or some related influence may help in the faculty as well. The direction, speed, temperature and other qualities of the wind seem to play a large part in this faculty in the case of a bird flying from one town to another. A useful list of books which treat of these subjects up to the present time is to be found at the end of the article.

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EXPERIMENTAL.

Ueber die Beeinflussung der Auffassungsfähigkeit durch einige Arzneimittel. NARZISS ACH. Psychologische Arbeiten (Kraepelin). Bd. III., 2 Heft, 1900. S. 203–288.

The writer subjected various medicaments to systematic test with a view to a quantitative estimation of their influence upon the process of perception and the capacity for retention. The drugs used were alcohol, bromine, paraldehyd and coffee. The work was done in 1897, part at Heidelberg, the rest at Strassburg. The method followed was that employed by Cron and Kraepelin in analogous experiments. Three visual series were read through at each exercise, each series consisting of nonsense syllables, monosyllabic words and disyllables. In each experiment the percentage of errors of various kinds occurring in the reading of the material, presented by a revolving drum, was compared with percentages occurring when the reacting subject was under the influence of the drug. In the case of coffee the dose was administered half an hour before the beginning of the experiment, normal and drugged conditions being tested on separate days. In experiments with the remaining drugs both phases were explored at the same exercise, the drug being taken at the close of the normal period and a pause of ten minutes occurring before proceeding with the rest of the drug state. The use of alcohol was avoided throughout the investigation, and that of coffee within six hours of the test.

As concerns their influence upon psychic processes the drugs fall into three classes. The first comprises alcohol and paraldehyd, the effect of which is injurious; the second, bromine, affects the activities appreciably in neither direction; while coffee, taken in moderate quantities, works an undeniable improvement in the perceptive faculty.

The influence of the first group is shown in a lowering of the capacity for work. This appears in two forms: first, an increase above the normal in the number of elements which are overlooked

and omitted; and second, an increase in the number of misreadings. Deterioration in the perceptive activity will be most unequivocally exhibited in those processes in which the factor of memory and association is reduced to its lowest value, namely, in the reading of nonsense syllables; and it is here that the increase in errors reported in the experiments was most marked. This effect of alcohol and paraldehyd had already been inferred by Kraepelin from his experiments in discrimination and reaction times. In the case of both of these drugs the group of letters which constitutes a word is no longer apprehended as a totality, but tends to break up into more primitive rhythmical combinations in which the first, third and fifth letters are favored, while the second, fourth and sixth are overlooked.

These drugs bring about both a retardation of the perception process and a narrowing of the span of apprehension, which produces a feeling of increased rapidity in the rate at which the letters pass before the opening. The successive phases of action in the two drugs present important differences. The effect of paraldehyd is more intense and more transient than that of alcohol. The impairment of perception due to the latter appears about ten minutes after taking the drug, and rises slowly to a maximum attained at the end of half an hour; while the action of paraldehyd is shown in seven minutes, and its acute stage is reached in fifteen to twenty minutes.

With increased practice the alcoholic interference passes rapidly away. This is especially noticeable in the reading of words, in which the association factor may play a great rôle. In general the memory suffered no apparent injury through the use of alcohol or paraldehyd, and the results may be regarded as harmonizing with those of Kraepelin, who found the former drug to bring about initially an increased facility in learning numbers. The prevalence of a reappearance of the same verbal combinations in mistakes made under the influence of these drugs is worth noting.

The experiments with bromine presented practically negative results. The action of coffee as evidenced by these tests is favorable to the activity of visual perception. Its effect appears especially in a reduction of the number of omissions, but is shown also in a lowering of the number of misreadings and misreproductions. With the quickening of apprehension goes also an increased accuracy of retention.

The temporal phases of coffee are not so marked as those of alcohol and paraldehyd. Its action is less intense than theirs, and more sluggish. Its effect becomes noticeable fifteen or twenty minutes after taking, and even after an hour and a half is still observ-

able. Here, again, the results of previous experiments are corroborated: Diehl and Vintschgau found a striking and long-enduring reduction of reaction times under the influence of coffee, and Kraepelin reports a facilitation of the apprehension of external impressions through its use, as well as the process of habitual association.

The results of these detailed experiments support the popular conception of the effects of this group of drugs and its medical application. Coffee causes a moderate rise in general mental activity, which but slowly passes away and is manifested in an increased capacity for work and pleasure in it, while at the same time it postpones the advent of fatigue and sleep. The value of bromine lies in a quieting of excitement characteristic of certain diseases, and its action seems a variable independent of the general mental activity. Paraldehyd produces a swift effect which shortly passes away, and is, therefore, adapted to the purpose of inducing sleep. The effects of alcohol manifested here are identical with those more striking forms of perversion and injury which accompany the drinking bout. The misapprehension of speech is here, the incapacity for analysis and discrimination, the premature fatigue, the uncontrollable desire for sleep. Moreover, it is the same functions which are most intimately interfered with here as in social drinking. The activity of selective consciousness suffers most, habit and the mechanical processes of the subject are less affected. In the pathology of chronic alcoholism this slowness and uncertainty of apprehension appear in extreme degree, yet it is but the extension and fixation of results which these finely quantitative experiments show to ensue upon even the very moderate use of alcohol.

Rhythmus und Arbeit. MARGARET KEIVER SMITH. Philosophische Studien, Bd. XVI., 1 Heft, 1900. S. 71–133.

Miss Smith's article is a report of work carried on during the year 1898 in the laboratory of Professor Meumann in Zürich. The account is incomplete, giving only the results of the experiments serially, and leaving for a later article the summary of the work and its bearing upon pedagogical and other problems. The object of the investigation is to determine by a series of simple mental tasks under what condition the carrying on of work rhythmically improves its quality or increases its quantity over that of similar work done arhythmically.

It is to be noted, however, that the experiments did not strictly set in contrast phases of activity which were respectively rhythmical and non-rhythmical, such as would be attained by carrying on both pieces

of work in obedience to objectively given periods, which were regular in the one case and irregular in the other; but compared work done under direction of a rhythmical set of signals with that performed freely in natural periods without conduct of any kind. The existence of rhythmical activity in the latter case is of course a possible hypothesis, and much evidence goes to confirm the belief that natural rhythms are widely characteristic of physical and mental work. As a consequence of the comparison of the results here reported with work done under control of arhythmical objective periods we should look for an increase of the margin of effectiveness which the writer found, within the scope of her experiments, to exist in favor of work rhythmically conducted.

The investigation comprised five problems, beginning with simple muscle work and proceeding through a series in which the mental factor was of progressively increasing importance. The first exercise consisted of ergographically recorded movements of the middle finger, both in extension and reflexion, continued until the appearance of complete fatigue. In the second group simple curves, letters and syllables were written continuously for a fixed period. The third set compared the accuracy in discrimination of slightly differing weights when made without and with observance of a rhythmically given signal. In the fourth the influence of rhythm upon the process of committing to memory series of letters and nonsense syllables was studied. The fifth and last group comprised a series of experiments in which the motor factor was reduced to the smallest attainable value, and the effect of rhythm upon 'pure visual' acquirement was investigated by a method devised to meet the conditions of the problem. Neither of the last two groups is reported upon in this paper.

The results obtained in the earlier series are in abstract as follows: In regard to the first group nothing satisfactory is to be reported. The work done without objective control so rapidly fell into a completely rhythmical form that a comparison of rhythmic with non-rhythmic activity was out of the question. The quality of the work becomes a little more regular under guidance of the metronome, and in some cases more work is done with than without it.

Half a dozen variations of the second exercise—writing letters and words—were gone through with, which showed a fairly constant, though slight, balance in favor of the metronome accompaniment. In connection with these writing exercises it is to be remembered that each person has his own characteristic tempo, and any computation of the influence of rhythm must take into account

the possible interference of the objective rhythm with this natural period. The conflict of these tendencies appears in a deterioration in the quality of the work done. When the experimenter loses his natural rhythm and is unable to fall in with the new period, the favorable influence of rhythm is lost, and its effect may be distinctly fatiguing. Indeed, on the basis of these experiments little can be said in general concerning the influence of rhythm upon the quality of the work done. Upon the quantity of work done rhythmical conduct shows a consistently favorable effect.

One set of experiments in which the unit written consisted of the word 'command' resulted in a curious failure. Both of the two subjects accomplished more work without the guidance of the metronome than with it. The result was apparently not due to any unnaturalness in the period of the metronome, since many varying rates were tried. The experiments were soon given up. The influences of rhythm have thus evidently wide individual variations, and it would be interesting to know more in detail concerning the periods in which these subjects naturally wrote. It is conceivable that here we are met with cases of complex natural rhythm of alternate phases instead of the absence of periodicity. To such a form of reaction no metronomic period, however varied, would conform.

The introduction of variations in the rate of the metronome from forty beats per second up to one hundred and sixty showed a fairly well marked maximum of effectiveness or fitness occurring within a range extending from eighty beats per minute to one hundred, in accompaniment to which the greatest amount of work was done.

The introduction of rhythmical guidance into the process of comparing lifted weights produced effects analogous to the preceding. It is marked by an increase in the percentage of correct judgments over that returned without such accompaniment. The gain is small, in one instance having a value of $99\frac{22}{48} : 97\frac{1}{47}$, in another that of 89.5 : 88.7, in a third showing no influence favorable or unfavorable.

Throughout these experiments the influence of the metronome apparently was shown in an attitude of increased expectancy and a heightening of the attention process. We should, therefore, possibly refer at least part of this small advantage of the rhythmically conducted exercise over the free activity, directly to a quickening of the attention process, which is independent of rhythmical functioning and may be aroused by any form of mentally exciting stimulus. Even when found unpleasant this increased tension of the perceptive or motor process was noticeable and promoted the tendency to increased production.

In a final series of experiments in which one subject took part the influence of movements free in extent as well as time was contrasted with that of reactions controlled both in period and extent. In his free movements the subject raised the weights a full meter high, and the exhaustive character of the exercise was shown in the enduring fatigue which ensued. When the upward lifting of the weights was limited to a movement ten centimeters in extent—as in all preceding sets of experiment—the subject became distrustful of his judgment and found the restraint unpleasant. Though the results were ambiguous the test fulfilled its purpose in that the free movements of the subject showed a well-marked tendency to maintain a rhythmical period in the lifting of the weights.

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Psychological Studies. By HARLOW GALE. Number one. July, 1900. Pp. 175. Published by the author, Minneapolis, Minn. For sale by H. W. Wilson, University Book Store, Minneapolis, and Berhn. Liebisch, Leipzig.

The first study on 'Our Nervous System and its Use' is a condensation into 38 pages of the author's four or more lectures and demonstration with original lantern slides, in which the object is to show the latest and most reliable pictures of the brain elements, their connections and their probable functions. The reason for this effort by a psychologist is because the modern anatomy and physiology of the nervous system are so scattered and difficult of collection, and because of the immense importance of some first-hand knowledge of the nerve elements for widening the too-often narrow bounds of the psychological student. For demonstration material in this article there are given 21 original photo-micrographs in half-tone and 29 reproductions of schematic figures and plates, with photographs by Flechsig, Cajal, and His. The summary of the article is as follows:

1. The means of taking in the stimuli surrounding our bodies is the sensory system of bipolar cells situated either within the end organ of sense or in groups (ganglia) at various distances between the sense organ and the cerebro-spinal system. The uniform plan of the sensory cells is one fiber growing out to the surface of the body to receive through the sense organ the stimulus, and the other fiber growing into the cord or brain to conduct the stimulus therein.

2. The cerebro-spinal nervous matter consists of: (*a*) intaking fibers of the outlying sensory cells, (*b*) motor cells with their outgoing

fibers and (*c*) a mass of cells serving to connect together the different sensory stimuli and to connect these associated stimuli with the outgoing motor cells, these different grades of interpolated cells having all the general function of association.

3. The embryological evidence confirms this simplified plan of three functionally different cells, viz., (*a*) sensory cells outside the cord and brain tube, (*b*) motor cells going out of the tube and (*c*) association cells within the tube.

4. The specialized function of the small brain is to make the important associations for our complicated motor reactions and expressions.

5. The specialized function of the large brain is to make associations of the stimuli from the surrounding world so that thinking and feeling result, whereby reactions are made with more prevision for our welfare.

6. The wonderful advance of the last dozen years in our knowledge of our marvellous brain cells has been in disclosing an inconceivable possibility for association, which is the great psychological basis of our mental life. So that with an ever-increasing wonder on seeing real pictures and details of this most intimate and beautiful mechanism, its earnest student can share some of the satisfying and yet stimulating strength of Flechsig when he concludes the preface of his epoch-making work—"More and more have I the conviction that the brain covers fully and completely the facts of the mental life as their organ, and that we are in a situation to develop the conditions of this dependence with the same exactness as in the other phenomena of nature accessible to our knowledge."

Connected with this article, though at the close of the volume, is a catalogue of 126 reproduced schematic cuts and plates, and of original photo-micrographs of nerve cells and tracts; prepared as lantern slides for lecture use in demonstrating the histology, physiology and psychology of the human nervous system. The 47 reproductions are from the original works of Retzius, Cajal, Golgi, Dejerine, Kölliker, Starr, Meynert, Hammerberg, Flechsig, Ranke and His; and the 79 original photo-micrographs are mostly from original preparations made in Professor His' Anatomical Institute in Leipzig, where a duplicate set of these slides are in use.

The second study, 'On the Psychology of Advertising,' gives the results of a two years' series of experiments in exposing in the dark room regular magazine advertising pages and artificially prepared ones to test the initial attention value of text matter as against cuts, of rele-

vancy and irrelevancy of each to the article advertised, of the effects of repetition on each of these four elements, of different colors, of different sizes of type and of different parts of the page. By means of answers to four sets of artificial advertisements, in which each article was presented in six different kinds of advertisements, an effort was made to get at the reasons which influence buyers in their choices. Any differences in the sexes were kept in view throughout. From the three figures and twenty tables, mostly taken on college juniors and seniors, there resulted the following conclusions:

1. Attention: (*a*) Best gained on the whole by relevant words or text matter, next by relevant and then irrelevant cuts, and least by irrelevant words.

(*b*) Through five repetitions the relevant words increased steadily in value, while the cuts decreased, and the irrelevant words remained on their initially low level.

(*c*) The female observers were attracted proportionally more than the males by cuts and by irrelevancy.

(*d*) In colors, males were attracted mostly by black and green, females by red and green.

(*e*) A progressive increase of attention within the sizes of type from 2 to 5 mm. high.

(*f*) The left side of the page was seen more than the right.

(*g*) The horizontal quarter above the middle was on the whole more effective than the other quarters, with the bottom quarter decidedly least valuable.

(*h*) Only about half our informants seemed to be influenced consciously by advertising; the females somewhat more so than the males, however. But a considerable amount of advertising acts upon people below the threshold of their conscious attention and memories.

2. The reasons given for choosing one advertisement over others as the place from which to buy were: (*a*) Decidedly foremost from the age or reliability of the firm; then from cheapness, statement of qualities, with statement of prices and style least effective.

(*b*) For last choice cheapness was by far the main reason, either directly or under the brand of 'fake,' while an unanalyzable distrust and suspicion from too great claims or details was next operative.

In 'The Vocabularies of Three Children of one Family to Two and a half Years of Age' are given in chronological order for each part of speech, the words for which our three children used some definitely associated sound. A detailed comparison of the method here used with the other published records is made to give some judgment on the wide

variations reported in the vocabularies at about two years. A detailed record is given of the different words and the total words used on a single day in each month. The bearing is then suggested of the evidence of these usually greatly underestimated vocabularies of children on the philological legends of an adult's vocabulary. A special comparison is made of the community as against the diversity in the vocabularies used by three children in the same family and surroundings. From these vocabularies, occupying 23 pages, and from the accompanying 8 tables, the following summary is made:

1. By a comparison of the methods and results in taking the vocabularies of our three children with others it appears that most of the published summaries have been greatly underestimated.

2. Owing to the weakness and illusions of memory the most trustworthy method to take a child's vocabulary is to record the new words as heard each day and then periodically to take a whole day's talk to check off with the chronological list.

3. The vocabulary of an average two-year-old child thus appears to be about 700 words; and in the next six months it doubles to about 1,400 at two and a half years.

4. A child uses from 50 per cent. to 65 per cent. of its entire vocabulary on any one day.

5. A child's vocabulary being so far larger than the popular guess or attempted estimate,—this makes one decidedly doubt the ridiculously small guesses of the philologists as to the vocabulary of adults.

6. The total talk of a child for a day—from 5,000 to 10,000 words—the number of repetitions of its most-used words, or even a verbatim report of its talk for a half-hour, shows its immense activity and its real education through this activity.

7. Children in the same family and environment show surprising individual differences in the vocabularies used; two such children having less than half their vocabularies in common and over a quarter of them exclusively.

8. The words making up a vocabulary, the order of its acquisition and its community or diversity with other children's seem chiefly accounted for by the pleasure-pain element attached to the hearing knowledge of the word. So that, if there is sufficient interest in anything to use its word which has been heard, the motor connections having been subconsciously made, some sound is used for the word without practice.

The study on 'Taste and Smell in Articles of Diet,' compiled and tested by R. W. Tallman, goes through a large number of foods,

spices, flavoring extracts, liquors and tobaccos to separate the knowledge coming through taste from that through smell. The conclusions are as follows:

1. Having eliminated sight and smell, together with the attendant suggestion, and having deducted the touch elements, there remain usually the four fundamental tastes of sweet, sour, salt and bitter. 2. The power of perceiving these four fundamental taste sensations is not the same. Sweet is most easily and invariably recognized under all and every condition. Sour comes next and is closely followed by salt, which was occasionally not detected in peculiar, unexpected combinations. The experiments with bitter were not as extensive as with the other three tastes, but it was evident that bitter was not very readily recognized. 3. Individuals differ in their thresholds to each of the four tastes; what was quite sweet to one was not sweet to another. In sour the difference was not so marked, but existed, as it did also in salt and bitter. 4. Our perceptions of the taste of the various things we eat are greatly influenced by suggestion. It is remarkable how much one will honestly claim he tastes or smells, when, as a matter of fact, it is all due to suggested associations. 5. The sense of taste is greatly influenced by touch. 6. But the most important, conclusive and interesting result was the clear evidence obtained that almost all our so-called 'tastes,' or properly flavors, are due to smell. We have thus been acquiring a considerable part of our knowledge and pleasure-pain in a very different way and by means of a very different sense than we supposed. This is worth pondering upon and suggests an interesting inquiry as to whether this is not likely to be the case in other lines of knowledge and feeling.

In a postscript by H. Gale, the four tastes are compared with the chemical constituents of our foods, and a genetic explanation suggested, with some evidence from children, of the perception of difference in tastes and of the pleasure-pain therewith connected.

The study on 'A Case of Alleged Loss of Personal Identity' is that of an Englishman about 50 years of age, who appeared in Mankato, Minn., in October, 1896, and with very few memories of his past life. Although these lost memories gradually returned through a point of attachment and association he recalled nothing in connection with his frauds in a large lumber business in which he had enlisted Chicago capital and the collapse of which four months before had been followed by his loss of memories. In some dramatic interviews with his former Chicago partner he showed no memory or sign of guilt. An effort was made by suggestion in hypnotism to recover

these lost memories, but in vain. It was found however that he had had a similar mental lapse of shorter duration a year before, during which he was at Ward's Island Hospital, New York, and no dishonesties were connected with this first lapse of memories. In a long series of business schemes it was found that he had been an indefatigable worker, of temperate and thrifty habits, and there were many testimonials to his high personal character and to his activity in the Episcopal Church at various places. Naturally, however, the offhand judgment on the case was that his loss of memories had been feigned to cover his dishonesties. The study concludes, however:

"When one now looks back at this strange mental composition to analyze and classify it there appear to me two important characteristics connecting him over toward the insane.

"1. A delusion of grandeur ('Groessewahn') as shown in his restless activity and pleasure in these series of large schemes started on such small foundations. His absorption in them was intense, leaving him no interest for other things except his church. In my interviews with him he could not get away from talk about himself; some brain micro-photographs, which I, for this purpose, showed and explained to him, had no interest. In his church even he pushed himself forward everywhere in an unusually commanding way.

"2. The common delusion of persecution was an easier explanation of his series of failures than the reasonable and evident lack of foresight, capital and honesty. But no such reasoning would affect him. The reversed judgment of his friends, who came to know him thoroughly, was explained too in this way.

"With these two characteristics, which are only exaggerations of the normal mental components of pleasure in activity and success and of suspicion, is specially connected the question of his consciousness of and responsibility for his dishonest practices, however great or small they really have been. That he is not a deliberate or conscious knave, or even deceiver, seems to me reasonably sure. And as therefore the ordinary motives which influence the average mental complex do not come into his consciousness with inhibitory or motor force, he is not responsible as is the average man. His case is of much interest as an example of the many components of knowledge, feeling and motor reaction, which constitute the manifold complex 'mind,' and the limitless variety of these combinations, with the wide transition region between sanity and insanity. In its non-responsibility the case illustrates the modern 'necessity' of willing as a reign of law in mental life as against the old metaphysical 'freedom.' His would be a case for restraint and not punishment.

"As to the genuineness of his loss of personal identity, it seems to me in some degree at least genuine, and perhaps entirely so. His unstable nervous temperament, with its excessive activity and disposition to worry and suspicion, was favorable to such a catastrophe. For the first collapse in August, 1895, though only of short duration, there seems no motive for deception except to gain a retreat and rest. And if the first one was genuine, the still greater stress of circumstances which culminated in June, 1896, was enough to cause a greater mental rupture. However, the suspicious limits of his recovered memories remind one of the often wonderful ingenious reasoning and cunning self-control of the insane, and give much evidence to the probability that with some real loss of memories he consciously or unconsciously—as it seems to me more likely—used his more or less mental break to cover his dishonesties."

The last article on 'Psychical Research in American Universities' gives a report of any work done in our universities in the group of subjects vaguely embraced under psychical research and of the judgment or attitude of American psychologists toward this work. A plea is made for its usefulness in an education of culture; and for convenience of use there is appended a note of references to psychical research phenomena, aside from telepathy, in the publications of the Society for Psychical Research.

Thus, as the preface explains, these Studies are largely of a personal and popular character. But it is hoped that this character may not prevent any scientific work or value there may be in the Studies from contributing somewhat to the progress of our knowledge in this exhaustless psychological field.

AUTHOR'S ABSTRACT.

NEUROLOGICAL.

Theoretisches zur Tierpsychologie und vergleichenden Neurophysiologie. Prof. Dr. HEINRICH ERNST ZIEGLER. 'Biologisches Centralblatt,' Band XX., Nr. 1. 1 January, 1900. Pp. 1–16.

After entering a protest against according reflexes and instinct to animals so low in development as to be without (obvious) nervous systems, and the brief discussion of certain biological terms, the author, a zoölogist at Heidelberg, suggests the manner in which the neurons may develop with mental and psychophysical acquirement and incidentally, as it were, advances the terminology of neurology in a useful way. He accepts as valid the claims of Bethe, Apáthy,

Schultze, Nissl, etc., as to the 'primitive fibrillæ' extending from cell to cell.

Bethe, Beer and Uexküll have termed 'antikinesis' that nervous energy traversing the reflex arc, calling it a 'reflex' when it always travels the same path, but such currents as are modified by previous occurrences, thus going differently, they term 'antiklisis.' Of the first variety are the inherited coördinations and associations which in the individuals of a species are practically the same: reflexes and instincts. These Dr. Ziegler would have called '*kleronomic*' or inherited, paths, while such paths as are opened in the neural maze by the acquirements and experience of the individual he would term *enbiontic*—those associations and coördinations which have been learned and which are not, as Weismann seems to have shown, inheritable.

"The cells of the neural apparatus are joined together by the end-twigs of the branches in the neuropile and continue as the fibrillæ which extend within the cell-body from the end-twig of a cell to another end-twig of the cell," that is, in fine, are association-fibrils within the cell as well as intercellular, as Bethe and others have shown.

As to the origin of the kleronomic (inherited) and enbiontic (acquired) paths, Professor Ziegler has this to say: It is no more difficult to conceive how the former are produced than to see how any paths of the ontogeny are made. The latter, enbiontic, paths are not so easily explained. "Think of a complex of cells which are bound together in various ways by the end-twigs of the branches and through whose cell-bodies a great multitude of neurofibrils pass; these fibrils may be present already as weak and very fine structures and extending in many directions make all possible combinations, so to say, of connections between the branches; in case a network of fine fibrillæ exists it is the same. But in the case of the central organs where numerous sense organs are in connection receiving different sorts of stimuli, continual agreement of the stimuli from the outer world will stimulate single paths. This agitation as a functional stimulus produces a strengthening of the path related to it, thickening certain numerous paths through the fine neurofibrils, as also certain of the end-twigs. This in particular will be the case when the same combination of stimuli is frequently repeated, as also whenever the same object or occurrence is frequently experienced."

Wernicke, Mann, Biedermann and Bethe, the author admits, have approached this same view, but have never made it explicit. The article is illustrated by drawings showing the neurons as they appear in the various stages described.

Meragia Paræsthetica. J. H. Musser, M.D., and Joseph Sailer, M.D. Journal of Nervous and Mental Disease, Vol. 27, No. 1, January, 1900. Pp. 16–40.

Meralgia paræsthetica is the name given by Roth to a newly described 'neurosis of a nerve,' namely the external cutaneous nerve of the human thigh. This article gives a general description of the 'disease,' and adds account of ten cases to the three or four score already reported. The condition is of interest to the psychologist especially because it provides a striking illustration of the large number of sensational variations which may be correlated with disturbance, functional or structural, or both, in a single small nerve. The writers of this article define the 'neurosis' as 'a disturbance of sensation on the external surface of the thigh, characterized by various forms of paræsthesia, associated with dissociation and more or less diminution of sensation.' Its etiology and pathology are as yet probably unknown, while cure has not as yet been accomplished. It seems more similar to 'neuralgia' than to anything else, but why it should be confined to one limited portion of the external nerves is quite mysterious.

Among the subjective manifestations of the affection described by various observers may be mentioned these: pain of very many sorts; hyperæsthesia to touch and to pressure; hypæsthesia, or lessening of sensations of touch, pain and temperature, and of power of localization; insensitivity to electricity; numbness and prickling; burning sensation, coldness, delayed sensory perception, feeling as if flesh were loose, sensation of moisture.

The condition seems to be associated with a lowering of the general bodily tone, but is not generally more common in hysterics than in those of less neurotic habit. This is a suggestive example of what may be correlated in consciousness with wholly invisible changes in a portion of a small nerve or in its central organs.

George V. N. Dearborn.
Tufts College, Medical School.

NEW BOOKS.

Foundations of Knowledge. Alexander Thomas Ormond. London and New York, The Macmillan Co. 1900. Pp. xxvii + 528.

Tafeln zur Geschichte der Philosophie. C. Stumpf. Berlin, Speyer & Peters. 1900.

Ueber Psychologie der individuellen Differenzen. W. Stern. Leipzig, Barth. 1900.

The Life, Unpublished Letters and Philosophical Regimen of Anthony, Earl of Shaftesbury. Edited by Benjamin Rand. London, Swan Sonnenschein & Co.; New York, The Macmillan Co. 1900. Pp. xxx + 535.

NOTES.

We record with deep regret the death of Dr. Henry Sidgwick, Knightbridge professor of moral philosophy at Cambridge, which occurred on August 28th.

Professor Thomas Davidson, well known for his contributions to philosophy and education, died on September 19th, at the age of sixty years.

In the death of Dr. Harold Griffing psychology loses a student of much ability and promise. Dr. Griffing's inaugural thesis at Columbia University, 'On Sensations of Pressure and Impact,' was issued as the first Monograph Supplement of the Psychological Review, and he was the author of other valuable contributions to psychology, published in this Review, in the *American Journal of Psychology* and in the *Philosophical Review*. It deserves to be put on record that Dr. Griffing had dependent on him for support a mother and an invalid sister, and was consequently unable to accept such university positions with small salary as were open to him. He was obliged to undertake the practice of law, but it was his intention to return to work in psychology when circumstances permitted.

Professor W. James is still abroad but may return soon to Cambridge. His health has improved.

Professor George T. Ladd has returned to New Haven after a year's absence spent chiefly in Japan and India, where by special invitation he delivered lectures on philosophy and education at a number of the leading universities.

PROFESSOR J. G. HIBBEN, of Princeton University, is spending the year abroad and is at present in Strasburg.

PROFESSOR ROBERTS LATTA, lecturer in logic and philosophy in the University of St. Andrews, has been appointed to the chair of moral philosophy in the University of Aberdeen, vacant by the transfer of Professor Sorley to the corresponding chair at Cambridge University.

RICHARD K. PIEZ, Pd.D. (New York University), has been appointed professor of psychology at the State Normal School, Oswego, N. Y. Dr. Piez assumed the duties of his chair upon his recent return from a special tour in Europe, in which he made a study of the applications of modern pedagogy in the actual work of continental schools. Pitt. P. Colgrove, Pd.D. (1900), has resumed his duties at the State Normal School, St. Cloud, Minn., after leave of absence extending over two years, which he spent in study at New York University. Dr. Colgrove will have charge of the departments of psychology and mathematics.

A *Revue de philosophie*, under Catholic auspices, will be published bi-monthly at Paris, beginning November 1st. It will be edited by M. R. P. Peillaube.

PROFESSOR E. C. SANFORD'S Course in Experimental Psychology has been translated into French by Dr. A. Schinz.

PROFESSOR W. JAMES asks us to state that the appearance of his name as member for America of the Council of Organization of the 'Institute Psychique International' is unauthorized.

IN printing Dunlap's 'Effect of Imperceptible Shadows on the Judgment of Distance,' in the last number of this REVIEW, an error occurred in designating several of the curves. The two curves on p. 449, marked Figs. 7 and 8, belong to the group on p. 451 and should be designated Figs. 11 and 12, respectively; and the two printed as Figs. 11 and 12 should be carried over to the group on p. 449 and be marked Figs. 7 and 8. In the same 'studies' the printers insisted on using 'visible' in place of 'visibile,' although the error was corrected in the proof.

ALL communications for the editors of the PSYCHOLOGICAL REVIEW should be sent during the year beginning November 1, 1900, to Professor J. Mark Baldwin, Princeton, N. J.

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